

PHYSICAL SCIENCES

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PHYSICAL SCIENCES

Volume 1

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PHYSICAL SCIENCES

Volume I

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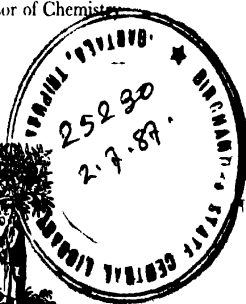
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PREFACE TO THE PHYSICAL SCIENCES

In this series, the Discourses dealing with Chemistry and Physics have been grouped together under the one heading. It is difficult to draw a sharp line of demarcation between Chemistry and Physics, especially in the earlier Discourses where such a distinction was hardly recognized. Further, many of the Discourses deal with subjects which are of fundamental interest both to Physics and Chemistry. This is especially so in the present century when the structure of the chemical properties of the elements came to be understood. No attempt has been made, therefore, to arrange the Discourses under the separate headings of Physics and Chemistry, because if this were done each series would be so incomplete.

The period which is covered, 1851 to 1939, witnessed the great change from "Classical" Physics to the "New" Physics. It is generally acknowledged that J. J. Thomson was responsible for initiating this revolution, by his identification of the electron as a body much smaller than an atom and a constituent of all atoms. The first announcement of his discovery to the scientific world was his Discourse on "Cathode Rays" at The Royal Institution in 1897, a famous occasion in its history.

Before then it was the day of Classical Physics, and the Discourses reflect the interests of that period. A structure of imposing symmetry and completeness had been built on the foundations laid by famous men in the fields of Heat, Light, Sound, Electricity and Magnetism. It was a time when most of the fundamental discoveries of Physics seemed to have been made, and all that remained was "to measure the fundamental constants of Nature to another decimal place". Many of the Discourses were on subjects which may seem now to be of relatively minor importance, but they are fascinating to read because, although they often deal with points of detail, they reflect such a deep interest in manifestations of scientific principles. There are many Discourses by Faraday and Tyndall who were Professors in the Institution, and by others who have made scientific history—Andrews, Stokes, Cornu, Crookes, William Thomson,

Rayleigh, Dewar, Tesla, Ewing, John Hopkinson, Lippmann and many others.

Then, starting with J. J. Thomson's account of his "corpuscles" (electrons) the Discourses assume a new character because a new world of Physics had been discovered. Becquerel and Curie describe the discovery of radioactivity, and there are many Discourses by Rutherford. C. T. R. Wilson describes his Cloud Chamber, Soddy talks about isotopes, Barkla about X-rays, Zeeman about magneto-optics and Jeans explains the significance of the new mechanics.

In chemistry as well, the period covered by these Discourses witnessed a transformation of the science. During the two decades following the year 1851 the basic ideas of structural organic chemistry and valency were laid down and the lectures of Williamson, Odling, Frankland, Mendeleev, Crum-Brown and others illustrate in a fascinating way, how difficult was the birth of these concepts which to-day seem so elementary. Hofman describes the first synthesis of the dyes mauve and magenta, harbingers of a great chemical industry. The properties of new metallic elements are described almost as they are discovered and Rayleigh gives an account of his discovery, with Ramsay, of the first of the noble gases, argon. Lord Rayleigh was a Professor of the Royal Institution and the breadth of his scientific work, described in many Discourses, is quite remarkable.

The foundations of much of physical chemistry were laid during this period. Arrhenius describes his development of the theory of electrolytic dissociation, Freundlich, McBain and Langmuir lay the basis of colloid science and the development of chemical spectroscopy can be traced through the Discourses of Stokes, R. W. Wood, Roscoe and others. Dewar describes his preparation of liquid and solid hydrogen in the laboratories of the Institution and Sir William Henry Bragg, who directed the Institution during the last years of this period, gives many accounts of the development and applications of X-ray analysis.

This series of Discourses therefore represents a cross section of the growth of physics and chemistry in the latter half of the nineteenth century and the first half of the twentieth century and represents a great turning point in the history of the physical sciences.

WILLIAM LAWRENCE BRAGG
GEORGE PORTER

FOREWORD TO THE SERIES BY THE GENERAL EDITOR

Sir William Lawrence Bragg

The discourses at the Royal Institution are unique in character. On each Friday between October and June, a well-known authority is invited to give a general review of his subject, and he is asked not to assume any expert scientific knowledge on the part of his listeners. A tradition has grown up around the "Royal Institution Discourse" which is regarded as a special occasion calling for a talk distinguished by simplicity and clarity and, when appropriate, illustrated by interesting experiments and demonstrations. Most of the discourses are on scientific subjects, but the arts are also represented.

These talks present a broad survey of current scientific thought and achievement. They carry the authority of the famous men who have given them, and at the same time they are easy to understand. Their great interest provides the justification for their being published in the present form. No attempt has been made to select; all have been included because it is rare to find one lacking in interest today.

Since the number of discourses is large and they cover such a wide range, they have been grouped under main subject headings and the discourses under each of the following headings will be published as a separate series:

The Physical Sciences	(Physics and Chemistry)
Astronomy	(Astronomical Sciences)
The Earth Sciences	(Geography, Geology, Meteorology)
The Biological Sciences	(Botany, Zoology, Palaeontology)

After the above volumes have been published, it is anticipated that further volumes in the series will appear under the following headings:

The Medical Sciences	(Medicine, Physiology, Psychology)
The Social Sciences	(Anthropology, Archaeology, Sociology, Economics)

The Applied Sciences

(Electrical, Mechanical,
Chemical and Civil Engi-
neering, Metallurgy)

The History and Philosophy of Science

The series starts in the year 1851, because it was only then that regular publication of accounts or abstracts of the discourses began. The Royal Institution at this date had already been a major centre of research in Great Britain for fifty years, first under Humphry Davy and then under Michael Faraday and many famous men had worked there. A decision as to when to end the series must be arbitrary, but it was felt that the more recent discourses, while just as interesting, do not have the same attraction of catching the atmosphere of science in generations which have passed which is much of the charm of the earlier ones. This series was therefore brought to a close in 1939, just before World War II.

It is hoped that this series will be interesting to professional scientists, and especially to students of the History of Science in that it preserves so much fascinating material in a readily available form. But it is also hoped that this collection will be welcome to all who are interested in science since it was to them that the Discourses were originally addressed and for them that they were so carefully planned by the great masters in the past.

Friday, January 24.

SIR R. I. MURCHISON, Vice-President, in the Chair.

PROFESSOR FARADAY

On the Magnetic Characters and Relations of Oxygen and Nitrogen.

IN a Friday Evening discourse on the diamagnetic condition of flame and gases, delivered on the 14th April, 1848, Mr. Faraday called attention to the singular condition of oxygen gas in its relation to the magnet. It was then demonstrated that this gas was magnetic by its carrying a cloud of muriate of ammonia (itself diamagnetic) to the poles of the magnet, around which it seemed to gyrate in vortices. A more elaborate paper on the same subject had previously appeared in the Phil. Mag. for December, 1847.

Last year M. Becquerel, not aware of these researches, had rediscovered the high magnetic character of oxygen, made some independent investigations, and derived numerical results from them. These inquiries Mr. Faraday does not consider to interfere with, but strongly to confirm his own.

Oxygen is one of the most remarkable of known bodies: it forms one half of the aggregate of all matter. Important as are its magnetic properties, it seems incapable of receiving permanent magnetism like steel or the natural loadstone. — By a series of elementary experiments the audience were led to discriminate between these bodies, and soft iron, nickel, cobalt; which unless while under an extraneous magnetic influence, have no attractive force. Oxygen being of the latter class, it is not certain that, even while it possesses an attractive power, it is in the exact condition of the permanently magnetical body from which it derives it.

Were oxygen highly magnetic in the same extent as iron is, the immense quantity of magnetic power which would in that case be constantly undergoing variation by combustion, respiration, &c., would cause the most serious disturbances in nature. It is necessary to the conservation of the present state of things that the magnetic power in a given bulk of oxygen should be comparatively small. The audience were therefore told to expect no great demonstration of magnetism; but the extent to which that power does exist in oxygen and air, was proved by the following experiments: —

A double cone of iron (the apices of the cones meeting in a point, and the cones being equal and similar,) was fabricated of such a length as to complete the magnetic circuit when placed between the poles of the large electro-magnet in possession of the Royal Institution. Mr. Faraday directed attention to this hourglass-shaped piece, and showed how, by such an arrangement, extreme power is exerted at the place without any chance of change in the form of the parts. Very small soap-bubbles were blown by means of a glass tube drawn to a fine point, from a bladder filled with oxygen. It was observed that these bladders so filled were drawn forcibly inwards to the apices of the cones, but that no such effect followed when bubbles were filled with nitrogen. Another experiment, which was visible all over the room, at once demonstrated the same fact, and illustrated a differential mode of measuring the magnetic force of oxygen. A delicately balanced wire was suspended from its centre of gravity by 10 fibres of the cocoon of the silk-worm; from the extremities of a small cross bar at one end of this wire were hung small glass bubbles; and the whole was so adjusted that the bubbles were on opposite sides of the apices first described, each hanging near to it but not in contact with the iron, and each equidistant from it. Therefore any difference of magnetic influence on the bubbles or their contents would be indicated by the bubble so affected being drawn inwards. In order to render such motion widely visible, the other arm of the balance just described was converted into a long indicating lever, constructed of a straw for the sake of lightness. To the extremity of the longer end a slip of silk was attached to catch the eye, and the lever was shielded from the currents in the room by being placed within a glass balloon two feet in diameter. By the motion of the lever it was seen, when one of the bubbles was filled completely or partially with oxygen and the other with nitrogen, that nitrogen, whether dense or rare, was totally unaffected by the magnet, and that oxygen was magnetic in direct proportion to its density in the bubble; and that the force required to set the bubble of oxygen (one atmosphere) in motion towards the magnet was one-tenth of a grain for one-third of a cubic inch of oxygen.

Certain peculiarities in the exertion of the power which is here in action, not as a central, but as an axial force, were then referred to.

The inference from the experiment, supported by other experiments on bubbles containing air, is — that as oxygen enters into the atmosphere in a constant proportion, and as the magnetic power of oxygen varies directly with its density, definite variation must take place in the magnetic power of the atmosphere in different states.

Mr. Faraday was led to inquire whether any separation of oxygen from nitrogen in a mixture of these gases could take place, as happens when a magnet is presented to a mixture of iron filings and sand. To test this idea he applied to the conical angle (so often de-

scribed as the centre of magnetic force.) a glass tube drawn to a point (as in the apparatus used for blowing the delicate soap-bubbles,) and filled with water; by slowly withdrawing the water, the air could be drawn into the tube from any desired spot and tested. This was done; and it was found that even when the magnetic action was most intense, the proportions of the magnetic oxygen and of the non-magnetic nitrogen were undisturbed.—The following experiment proved that no condensation was produced on oxygen by the magnetic power, *i. e.* that it is not aggregated, as happens with iron filings when under the influence of the magnet. The flat-faced poles of the magnet were separated the 60th of an inch by a copper plate with an aperture in the middle, so that when the whole was clamped together a chamber was formed. By gauges attached to this chamber it was found that no trace of condensation occurred, however great was the magnetical force brought to bear on the oxygen.

The loss of magnetical power occasioned by heat was then noticed. This was shown first in the case of iron heated to redness; then in that of nickel raised to the temperature of boiling oil; and lastly, in the case of the air (*i. e.* of the oxygen in this air) by the following experiment:—Two conical poles a little separated were employed; above was placed a piece of phosphorus on paper, and below a helix of platinum wire heated to redness by a small Grove's battery independent of that used to excite the electro-magnet. The heated air, rising upwards from the helix, speedily inflamed the phosphorus above it whilst the electro-magnet was unexcited; but when rendered active, the oxygen in the heated air becoming less magnetic, was displaced by the current of colder (and consequently more magnetical) oxygen, and the phosphorus in consequence remained unaffected by the mass which glowed beneath it, until the electro-magnet was deprived of its power; and then the natural laws of specific gravity came again into operation, the heated air rose, and the phosphorus was lighted.

In conclusion, Mr. Faraday announced his intention of applying, on a future evening, the reasoning deducible from these and other experiments, to the variation of magnetic lines on the earth's surface. His purpose then will be to compare the records of this varying force with the variations of temperature occasioned by the annual revolution of the earth, the varying pressure of the atmosphere, storms, &c. with the hope of supplying a true theory of the cause of the annual and diurnal, and many of the irregular variations of the terrestrial magnetic power.

For the papers in which these results are described more at large, see *Philosophical Magazine*, 1847, Vol. xxxi. p. 401; and *Philosophical Transactions* for 1851, p. 1.

Friday, March 28.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer, in the Chair.

NEVIL STORY MASKELYNE, Esq., M.A.

On the Connexion of Chemical Forces with the Polarization of Light.

ANY facts which can throw light upon the ultimate molecular structure and condition of chemical compounds, cannot fail of possessing interest of a high character, as well for those whose thoughts only casually dwell upon questions of physical science, as for the mathematician and the chemist. To the mathematician, indeed, they would, if completely unfolded, supply the data for him to undertake the resolution of the questions of chemical combination and chemical change, by treating them as problems involving the action of mechanical laws; to the chemist, the acquisition of such knowledge would be the removal of some of the profoundest difficulties of his philosophy: but such knowledge is only to be sought in the most difficult paths of the whole range of science. The question of the connexion of chemical type with crystalline form, the fruitful cause of so much contention among mineralogists as to the questions of mineral species, is one on which we have no complete and sure knowledge; for the facts of dimorphism show, that implicated with this question are the actions of other forces, such as electric condition, and above all the mysterious molecular alterations induced by heat. Another direction in which such inquiries have been pursued, has been in tracing the phenomena resulting from the property possessed by many bodies, of modifying a plane-polarized ray of light, by what is termed circular-polarization. This property, from its being proved to be, in a large number of cases, an expression of the *molecular* structure of the substance, and as such inseparable in many cases from its chemical existence, may be taken, whenever this can be shown to be the case, as an evidence of its individuality, and may be used to determine the question of the permanency or transitory character of the molecular type of the substance. The information thus gained may be but vaguely defined, and the truth but darkly seen, yet does it nevertheless afford a valuable and interesting point of view for studying the molecular nature of bodies.

M. Biot has been for forty years enriching chemico-physical science by a series of memoirs detailing the results of his study of these phenomena. He has there shown the value of this means of tracing changes in chemico-molecular constitution.

M. Pasteur has carried forward this inquiry into a new channel by tracing a connexion between this property in substances, of circularly polarizing light, and their crystalline character.

But as it would be impossible to explain the nature of his investigations, or their results, without a preliminary knowledge of the meaning of the terms "circular polarization," and "hemihedrism," it was necessary first to enter a little upon the explanation of them.

Accordingly a ray was explained as being a direction of light, having no relations to space which differed from each other in directions perpendicular to its length. Thus without complicating the subject, by using the language of the beautiful wave-theory, a ray might be imagined as a cylinder of minutest diameter but indefinite length. When such a ray is reflected at a certain angle from glass or such like substance it is split into two; one going into, and through the glass if it be not opaque, the other being reflected from it. These two rays no longer possess the same "absence of sides" as the original ray. For the one has been as it were flattened down to a "strip," while the other has also been flattened similarly into a "strip," but the latter strip is at right angles in regard to its "flattened plane" to what the other is. A similar bifurcation of the ray is produced in the interior of what are called doubly refracting crystals. This bifurcation and flattening of the ray is termed "plane polarization" of it; and it is so far a true instance of polarity—as that the two rays have equal and similar properties in opposed directions.

This was exhibited by the Lime-light. The double image of a small round hole formed by a crystal of Iceland spar was thrown on a screen, and each beam shown to be most capable of reflection in a plane in which the other was incapable of being reflected at all. The action of the tourmaline as a doubly refracting crystal which absorbs one of the rays was then explained; and it was shown that the position of the tourmaline in which it intercepted one ray entirely, was exactly the position in which it gave the other ray free passage. The optic axis of a crystal was then defined to be a direction in it along which the light could pass through the crystal without undergoing any change whatever. The central ray of a polarized beam of light, traversing a piece of calc spar along its optic axis, was shown to be intercepted or transmitted by a tourmaline, precisely as if the section of the crystal of calc spar were away.

An exception was however stated to exist to this law of the neutrality of the optic axis. When a section of quartz, cut so that the beam could career along its optic axis, was put in the path of the polarized ray, it was found that instead of permitting the ray to be eclipsed by the tourmaline when this was placed in the position

to eclipse it, that ray on the contrary fell on the screen endowed with beautiful colour; and furthermore that the revolution of the tourmaline induced the most brilliant succession of colours, in the order, in the instance exhibited, of red, plum-colour, blue, green, orange, red. It was shown, however, that another specimen exhibited these colours in the reverse order of red, orange, green, blue, plum-colour, red; in which order the former specimen of quartz produced these colours when the tourmaline was turned in the *opposite* direction. Hence these are termed right and left handed polarizations. The whole of these phenomena were attributed to a complicated set of movements of the light within the crystal, the *resultant* of which was practically a rotation of the plane in which the ray was capable of being reflected,—so that the thicker the crystal, the further round the tourmaline had to be turned to permit the ray to pass it, or to be eclipsed, as the case might be. The opposite order of the colours was explained by the fiction of supposing the one to be the effect of a left handed thread to the screw and the other of a right handed thread characterizing the spiral in which the plane of polarization was supposed to rotate. Of course this was only a popular way of explaining the phenomenon, it being really due to a more complicated series of movements which were explained by Fresnel in the most triumphant manner by the wave theory.

The colour was accounted for by the idea of the red following a longer spiral (having a coarser thread to the screw) than that of the orange, this than the yellow, and so on up to the violet. Without the tourmaline in front all would emerge and form white light; but the tourmaline only allows such rays to pass it as are capable of passing it in its particular position; *i. e.* only such, the rotation of whose plane has brought them round to the position of the plane in which the tourmaline lets the light through.

The singular fact of amethyst being a combination of alternate layers of right and left-handed quartz was then exhibited, both by throwing the image of the alternate layers on the screen, and afterwards by showing that the general effect of a traversing polarizing beam was to produce a neutrality of action. Other substances, however, produce phenomena of circular polarization. Uncrystallized, fused tartaric acid, and barley sugar, &c. produce them; and these bodies when dissolved, and many more in the form of liquids also, do so, some of which were exhibited. But the silica of which quartz consists entirely loses this property when divested of its crystalline character, whether artificially or in its natural state as calcedony, opal, &c. All other bodies retain it so long as their chemical molecule retains its individuality of character.

The next point to be made clear, was the meaning of that form of crystallographic developement called “Hemihedrism.” Haüy’s great law was, that similar edges or angles were always similarly modified. The nature of similarity in edges or angles was then pointed out, and the general idea of many crystallographers of a

sort of nucleus or primitive form existing on which the crystal was formed, was explained, as also the nature of the developement of such a crystal by the modifications placed on the edges and angles according to the law before mentioned. But the exception to that law was not less remarkable for its generality of character than the law itself. This exception consists in the fact that very often crystals are found in which not every similar edge or angle was modified, but where every *alternate* similar edge or *alternate* similar angle was so. This circumstance was then illustrated by the actual truncation of some models; and it was shown that such alternately developed or *hemihedral* crystals may be *right* and *left*, the upper terminal modification being to the right in the one case, and the corresponding and similar lower terminal modification being then to the left, while in the other case they are exactly the converse. Hence, one crystal is, as Pasteur describes it, "*non superposable*" to the other; the one is as the image of the other reflected in a mirror, as the right hand is compared to the left.

A beautiful connexion was then pointed out, as established long ago by the acute observation of Sir J. Herschel, that the plagiedral facettes of quartz indicated, by their relative positions on the crystal, the direction in which the crystal would rotate the plane of polarization. The crystallographic character of these facettes was then pointed out, and their connexion shown with this hemihedrism. But the most beautiful instance of the connexion of hemihedrism in crystals with the direction of the rotatory power of the substance of which they are composed is afforded by the recent discoveries of Pasteur, which may now be almost traced to a law, enunciated thus: that where a substance is hemihedric when crystallized, and possesses the rotating character, the direction of the rotation is indicated by the nature of the hemihedrism.

Paratartaric acid was then introduced. It was described as extremely like tartaric acid in its chemical reactions and identical with it in composition. It was similarly formed to the latter, and was found only one year in the cream of tartar of the wine of the Vosges. Pasteur separated the crystals of the Paratartrate of soda and ammonia into two several sets; the one set he showed to be hemihedric to the right, the other set to the left. The former proved to be the salt of an acid rotating to the right, the latter of one rotating to the left. On examining these acids they were found in every single property, but this rotation, identical with one another and with tartaric acid. Yet when mixed they formed again the Paratartaric acid, which, like the amethyst, is without any action of a rotatory character, and the difference of which from the other two acids was then exhibited by the precipitation by the latter of a salt of lime which did not render either of the former turbid.

Mr. Maskelyne then detailed the experiments of M. Pasteur on malic and aspartic acids and asparagine, and showed how all of these could be understood to contain chemically a molecular unit

common to all these and perhaps to tartaric acid, and only modified a little by the super-position as it were of other substances, in combination with it, upon the extremities of its molecule.

He also dwelt on the possibility of the Paratartaric acid being a quadribasic as the tartaric acid is a bibasic acid; it being on this view a conjugate acid consisting of the two united tartaric acids. He then invited attention to the interesting nature of M. Biot's investigation of the action of tartaric acid in solution in water, and he showed that here the acid must be supposed capable of combining with an indefinite or indeed an infinite amount of water, while in other cases again, bodies (such as sugar for instance) exercise no effect upon the water and do not seem to *combine* with, but only to be dissolved in it. The former is an instance of a *continuous* and not *intermittent* sort of combination; and though we need not anticipate a recurrence of the controversy of Berthollet and Proust, yet this shows us that the actions of quantity or mass so dwelt on by the former are not without a great significance; and that the power that can thus enable us to determine such important points in chemical statics, is well worthy of the attention of the philosophic mind.

Dr. Bence Jones permitted a Saccharimeter apparatus of Soleil's, on the double-quartz-plate principle, to be exhibited, and explained its use. Mr. Tennant also exhibited a mass of quite transparent Iceland spar, and a beautiful crystal of plagiedral quartz.

Since the delivery of the lecture, a letter has been received from M. Pasteur stating that he had forwarded for exhibition at and illustration of this lecture, all the finest specimens of the crystals which he has produced, which are further illustrated by models and diagrams. They are the same as those which were exhibited at the Academy of Sciences at Paris, and the liberality of M. Pasteur's act will be appreciated by Members of the Royal Institution, when they are reminded that the Paratartaric acid of which they are the products is impossible to be obtained, from its having only once been accidentally formed, and that these specimens therefore consist probably of the only large accumulation of this body in existence. As soon as they arrive they will be exhibited to the Members.

NEVIL STORY MASKELYNE.

Friday, April 11,

H. R. H. PRINCE ALBERT, Vice Patron, in the Chair.

PROFESSOR FARADAY

On Atmospheric Magnetism.

ON a former evening (*January 24, page 1*) it was shown that Oxygen gas was magnetic, being attracted towards the poles of a magnet; and that like other magnetic bodies, it lost and gained in power as its temperature was raised and lowered, and that the change occurred within the range of natural temperatures. These properties it carries into the atmosphere; and the object, this evening, was to show how far they might be applied to explain certain of the observed variations of the terrestrial magnetic force.

If a source of magnetic power be considered (as a magnet) it presents us with a system having polarity; and if the parts which are called the poles be taken as representing the most concentrated condition of the polarity, then the contrary polarities, manifest externally in relation to the magnet, are perfectly definite, being exactly equal to each other. If the magnet be irregular in the disposition of its force, still the same definite character of the sum of the contrary polarities holds good.

External to the magnet those concentrations which are named poles may be considered as connected by what are called magnetic curves, or lines of magnetic force, existing in the space around. These phrases have a high meaning, and represent the ideality of magnetism. They imply not merely the directions of force, which are made manifest when a little magnet, or a crystal, or other subject of magnetic action is placed amongst them, but those lines of power which connect and sustain the polarities, and exist as much when there is no magnetic needle or crystal there as when there is; having an independent existence analogous to (though very different in nature from) a ray of light or heat, which, though it be present in a given space, and even occupies time in its transmission, is absolutely insensible to us by any means whilst it remains a ray, and is only made known through its effects when it ceases to exist. The form of a line of magnetic force may vary exceedingly from a straight line to every degree of curvature, and may even have double and complicated curvatures impressed upon it. Its direction is determined by its polarity, the two changing together. Its powers are such, that a magnetic needle placed in it finds its place of rest parallel to it; a crystal of calcareous spar turns until its optic axis is transverse to it;

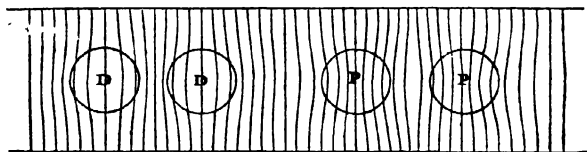
and a wire which is unaffected when moved in or along it, has an electric current evolved the instant that it passes across it : by these and by other means the presence of the magnetic line of force and its direction are rendered manifest.

The Earth is a great magnet : its power, according to Gauss, being equal to that which would be conferred if every cubic yard of it contained six one-pound magnets ; the sum of the force therefore is equal to 8,464,000,000,000,000,000 such magnets. The disposition of this magnetic force is not regular, nor are there any points on the surface which can be properly called poles : still the regions of polarity are in high north and south latitudes ; and these are connected by lines of magnetic force (being the lines of direction) which, generally speaking, rise out of the earth in one (magnetic) hemisphere, and passing in varied directions over the equatorial regions into the other hemisphere, there enter into the earth to complete the known circuit of power. A free needle shows the presence and direction of these lines. In London they issue from the earth at an angle of about 69° with the horizon (being the dip or inclination) ; and the plane in which they rise forms an angle of 23° W. nearly with true north, giving what is called west declination. Where the dip is small, as at the magnetic equator, these lines scarcely rise out of the earth and pass but a little way above the surface ; but where it is large, as in northern or southern latitudes, they rise up at a greater angle, and pass into the distant realms of space, from whence they return again to the earth in the opposite magnetic hemisphere ; thus investing the globe with a system of forces like that about an ordinary magnet, which wherever it passes through the atmosphere is subject to the changing action of its magnetic oxygen. There is every reason to believe that these lines are *held* in the earth, out of which they arise and by which they are produced, just as the lines which originate in a magnet are held by it, though not in the same degree ; and that any disturbance from above affecting them will cause a greater change in their place and direction in the atmosphere and space above, than in the earth beneath.

The system of lines of magnetic force around a magnet or the earth is related by a lateral tension of the whole, analogous in some degree to the lateral tension of lines of static electrical force ; both the one and the other being easily made manifest by experiment. The disturbance of the tension in one part is accompanied instantly by a disturbance of the tension in every other part ; for as the sum of the external powers of a system, unaltered at its origin, is definite and cannot be changed ; so any alteration either of intensity or direction amongst the lines of force at one place, must be accompanied by a corresponding change at every other. So if a mass of soft iron on the east side of a magnet causes a concentration of the lines of force from the magnet on that side, a corresponding expansion or opening out of the lines on the west side must be and is at the same time produced ; or if the sun, on rising in the east, renders all the oxygen

of the air on that side of the globe less magnetic and less able therefore to favour the transition of the lines of terrestrial force there, a greater number of them will be determined through the western region; and even though the lines of force may be doubted by some as having a separate existence such as that above assumed, still no error as to the effects on magnetic needles would in that case be introduced, for they by experiment would be and are the same.

The power of a magnetic body as iron or oxygen to favour the transmission of lines of force through it more than other bodies not magnetic, may be expressed by the term conduction. Different bodies, as iron, nickel, oxygen, conduct in various degrees, and not only that, but the same body as iron or oxygen conducts in different degrees at different temperatures. When space traversed by uniform lines of magnetic force is occupied by a uniform body as air, the disposition of the lines is not altered; but if a better conducting substance than the air is introduced, so as to occupy part of the space, the lines are concentrated in it, and drawn from other parts as shown by P. P. in the figure, or if a worse conducting substance is



introduced, the lines are opened out as at D. D. In both cases the lines of force are inflected, and a small magnetic needle standing in them at the inflected part would have its direction changed accordingly. Experimental illustrations of these changes in direction are given in Mr. Faraday's paper in the Philosophical Transactions for 1851, Part I. Par. 2843, &c.

Now this by the hypothesis is assumed to take place in the atmosphere. Supposing it all at mean temperature, the lines of force would have the direction determined by the arrangement of the power within the earth. Then the sun's presence in the east would make all the atmosphere in that region a worse conductor, and cause it to assume the character of D; and as the sun came up to and passed over the meridian and away to the west, the atmosphere under his influence would bring up changes in direction like those shown in either D or D; it would therefore manifestly set a needle in a given latitude in opposite directions as it passed by; and as evidently set two needles in north and south latitudes in opposite directions at the same moment of time. As the night came on and a temperature lower than the mean came up from the east and passed over, the lines of force would be inflected as in P or P., and a reverse variation of the needle to that which occurred before would now take place.

That natural effects of variation must be produced consequent upon the magnetic nature of oxygen and its daily variations of temperature is manifest; but whether they cause the observed variations, or are competent to do so, is a question that can only be decided after very careful enquiry. Observations are now made on the surface of the earth with extreme care in many places, and these are collated, and the average or mean result, as to direction and intensity of the earth's force, ascertained for every hour and season; and also many remarkable, anomalous, and extra results evolved. A theory of the causes of any or all of these variations may be examined first by the *direction* which the varying needle does or ought to assume, and then by the *amount* of the variation. The hypothesis now brought forward has been compared with the mean daily variation for all the months in the year at north and south stations, as Toronto and Hobarton, and at many others near to and far from the Equator, and agrees in direction with the results observed far beyond what the author anticipated. Thus the paths described by the upper ends of free needles in the north and south hemispheres should be closed curves, with the motion in opposite and certain directions, and so they are:—the curves described by needles in north or south latitudes should be larger in summer and smaller in winter, and so they are:—a night or cold action should grow up in the winter months, and such is the case:—the northern hemisphere ought to have a certain predominance over the southern, because of its superior temperature, and that is so:—the disposition of land and water ought to have an influence, and there is one in the right direction:—so that in the first statement and examination of the hypothesis it appears to be remarkably supported by the facts. All these coincidences are particularly examined into and stated in the Philosophical transactions already referred to. The next step will be to ascertain what is the amount of change in the conducting power of the air for given changes of temperature, and then to apply that in the endeavour to ascertain whether the amount of change to be expected is (as well as the direction) accordant with that which really occurs.

M. F.

Friday, May 9.

THE DUKE OF NORTHUMBERLAND, President,
in the Chair.

THE REV. BADEN POWELL, M.A. F.R.S. F.R.A.S. F.G.S.

SAVILLIAN PROFESSOR OF GEOMETRY IN THE UNIVERSITY OF OXFORD.

*On the Recent Experiment showing the Rotation of the Earth by means
of the Pendulum.*

THE experiment alluded to has been the subject of so much popular notice at the present time that it would be needless to go into a particular description of its nature or object. If fully verified, the result would however hardly amount to any more *palpable* proof to the *senses* than other astronomical phenomena afford; in this case, as well as in those, the conclusion is equally derived from *reasoning* on the actual appearances.

An idea of such an effect seems to have occurred long ago, and is mentioned in a paper in the Phil. Trans. 1742, No. 468, by the Marquis de Poli, in the course of some observations on the pendulum of a different kind. He remarks, "I then considered (adopting the hypothesis of the Earth's motion) that in one oscillation of the pendulum there would not be described from its centre perfectly one and the same arc in the same plane:"—but he does not pursue the subject, as being foreign to his immediate object.

It appears also (see Comptes Rendus, 1851, No. 6) that in 1837 Poisson had hinted at such an effect, but supposed it of insensible amount.

To some minds difficulties present themselves in the first instance, which are easily removed by a few simple illustrations. In the first place the deviation from parallelism to itself, of the meridian of any place, during the rotation of the Earth, is a simple geometrical question easily determined, and the inclination expressed by a trigonometrical formula. In the next place the independence of the motion of the pendulum, notwithstanding that the point of support is carried along with the earth in its rotation, and that the whole seems to form a part of the earth, is a point easily elucidated by very simple experiments, in which the vibration of a small pendulum is seen to continue parallel to itself notwithstanding a motion given to the point of support; the effect being in fact only a simple consequence of the coexistence of two motions communicated to a body at the same time. A beautiful apparatus, lent by Mr Bishop for showing this, was exhibited on the present occasion.

The experiment originally made by M. Foucault was repeated and confirmed under the inspection of M. Arago, and other eminent scientific men with all due precautions in Paris, as also at Ghent, Brussels, and elsewhere. In England besides the public repetitions at the Russell, London, and Polytechnic Institutions, by Dr. Roget, Mr. Bishop, and Mr. Bass, the experiment has been tried at York by Professor Phillips, and at Bristol by Mr. Bunt, with careful attention to all the circumstances likely to ensure the avoidance of sources of error, and to secure precise results. At the Royal Institution on the present occasion the experiment was exhibited under two modifications by Dr. Bence Jones and by Mr. Bass. Other observers have also repeated it in various places, especially at Dublin, where Messrs. Haughton and Galbraith, Fellows of Trinity College, have pursued the research with all imaginable precautions, and have obtained results somewhat different from those of other observers. According to nearly all the other experiments the rate of deviation continued uniform: according to Messrs. Haughton and Galbraith, it varied: and they seem to have been the only observers who have watched through a complete revolution, the time of which was observed to be 28h. 26m.

The sources of probable error are numerous and not easy to be effectually guarded against. The most formidable, perhaps, is the extreme difficulty of causing the pendulum to vibrate truly in one plane, and to prevent its motion in a narrow ellipse. When this takes place, and the arc is considerable, the direction of the major axis is continually changing, owing to a well known mechanical cause (see Herschel's *Outlines of Astronomy*, p. 444); but this deviation is always in the same direction as that of the original motion of the pendulum, and consequently changes when that direction is changed. The true deviation may be distinguished from this, in that it is always from E. to W., independently of the direction of the original impulse; and the ball always passes accurately through the centre in every oscillation, whereas in the former case it never does.

For great accuracy, a variety of other precautions are requisite, as to the perfect freedom of suspension, guarding against currents, &c.; It is, however, possible that the elliptic deviation may oppose that due to the earth's rotation, while the latter may manifest itself in spite of the former.

It is extremely probable that many of the public repetitions may have been affected by these causes of error; yet some of those referred to have been made by men of so much eminence and experience as observers, as to render it highly improbable that they should not have been sufficiently guarded against every source of fallacy. The accordance of many of the results at different places within fair limits of error, is also a strong argument in favour of their accuracy and trustworthiness.

The rates of deviation for one hour as determined at different places do not seem to be more discrepant than would accord gene-

rally with the differences of latitude. The experiment at Paris gave about $11^{\circ} 30'$, at Bristol $11^{\circ} 42'$, at Dublin rather more than 12° , at York about 13° .

To apprehend the theoretical principle it is necessary to take into account, 1st, the simple inclination of two successive positions of the meridian of a place to each other after any interval of time: 2nd, the independence of the motion of the ball of the pendulum, of the rotation of the point of support: and, 3rdly, that the ball, though free in this sense, is not however wholly free, being continually drawn down by gravity in a direction *continually changing* (relatively to the original direction of vibration,) as the earth revolves. Hence, though from the second cause the ball would have a tendency always to preserve a motion parallel to its original motion, and thus to deviate regularly from the meridian, it will (from the third cause) not preserve this exact parallelism, but will take an intermediate direction. The exact determination of this direction cannot be made on any general considerations, but must be the result of detailed mathematical investigation.

Thus in general in any illustrative or analogous case, so long as the *axis* of vibration continues parallel to itself, the *arc* of vibration will continue parallel to itself; but if the *axis* do not continue parallel, the direction of the arc of vibration will *deviate*. This distinction has been laid down and illustrated experimentally, by Mr. Wheatstone.

The investigation as pursued by M. Binet (*Comptes Rendus*, 1851, No. 6-7,) as well as by other mathematicians, is primarily founded on the method long since proposed by Euler, of resolving the rotatory motion of one point on the earth's surface into two, one about the vertical of that point, the other about an axis at right angles to it: of which the latter is the part effective in determining the direction of gravity on the pendulum, and is proportional to the sine of the latitude of the point.

M. Binet makes this general theorem the foundation of an analytical investigation, in which the conditions of the motion of the pendulum generally are expressed by certain differential equations, the integration of which conducts him to certain expressions, which when simplified by the consideration of limiting the vibration to small arcs, gives the azimuthal velocity uniform in the direction from E. to W. and in a simple proportion to the sine of the latitude: giving therefore the deviation for one hour in the latitude of Paris about $11\frac{1}{2}^{\circ}$ and the time of a complete revolution $32^{\text{h}} 8^{\text{m}}$. An investigation has also been made independently by the Astronomer Royal, leading to very nearly the same result.

Other mathematical solutions have also been proposed by Dr. Day of Bristol, and by Mr. J. R. Young (late Professor of Mathematics at Belfast). The latter gentleman has obtained as a consequence of his investigations one remarkable result, which he states thus:

"The arc of the circular rim of the *table* subtended by the angle of deviation at its centre, is always (in one revolution of the earth) exactly equal to the difference in *length* of the two parallels of latitude described by the *centre* and *extremity* of the meridional diameter of the table." [See *Mechanic's Mag.* May 3rd & 10th, 1851.]

The lucid and able illustrations of the subject given by Professor Sylvester have thrown much light on the explanation.

Modifications of the principle have been suggested by M. Chasles, on the idea of the difference of rotatory velocity between any two points on the same meridian, which difference, insensible as it might seem to be for the minute length of a vibration, he shows, will in successive vibrations become sensible. This idea is nearly the same as that announced by Laplace (*Mécanique Céleste*, vol. iv. c. 5.), who infers a deviation in the plane of a projectile fired in the direction of the meridian. The same idea has been discussed also by other mathematicians: and has been further carried out by M. Poinsoot who has suggested, that if two balls suspended by separate strings, hanging together in contact, and consequently both partaking in the velocity of rotation of that point of the earth, were to be suddenly separated by releasing a spring placed between them, and at first confined by a string, they would then show the difference of velocity, belonging to points on the earth at that distance apart, and would consequently revolve round the vertical. (See *Comptes Rendus*, 1851, No. 14.)

A beautiful variation of the experiment has been suggested by Mr. Bravais (*Comptes Rendus*, 1851, No. 6.) in which a *perfectly* circular motion is communicated to a pendulum (by a peculiarly ingenious contrivance) the time of whose revolution will be different according as its direction conspires with or opposes that of the earth.

If all *torsion* in the thread could be got rid of, a ball simply suspended and furnished with an index in its equator would be seen to rotate. But the torsion destroys the effect. This is the suggestion of M. Baudrimont (*Comptes Rendus*, 1851, No. 8.)

But by far the most complete idea not only of the general principle, but of the precise law of the sine of the latitude, is obtained from the beautiful apparatus constructed by Mr. Wheatstone, in which the pendulum is replaced by the vibrations of a coiled spring, the axis of which can be placed in any required inclination or *latitude* with respect to a vertical semicircular frame, which is made to revolve about its vertical radius, and the direction of the vibrations is seen to change in a degree proportioned to the sine of the latitude or inclination; as for example for lat. 30 the sine = $\frac{1}{2}$: and consequently if the vibration be originally in the meridian, when the meridian has revolved 180°, the deviation = $180^\circ \times \frac{1}{2} = 90^\circ$ or is at right angles to the meridian.

This apparatus was exhibited at the lecture.

Upon the whole the experiment is one of high interest and importance: some discrepancies or difficulties in the different views taken of

the theory as well as in the observed results, seem to indicate that the subject, however apparently simple, has not yet been thoroughly worked out, — and to point to the desirableness of further repetitions of the experiments, if possible *in vacuo*, and with increased precautions, as well as to a revision of the dynamical and analytical processes, by which possibly any seeming difficulties may be cleared up.

B. P.

Friday, June 6.

THE DUKE OF NORTHUMBERLAND, President,
in the Chair.

PROFESSOR ALEXANDER WILLIAMSON,
UNIVERSITY COLLEGE, LONDON.

*Suggestions for the Dynamics of Chemistry derived from the Theory
of Etherification.*

THE human mind is only capable of understanding complicated phenomena when prepared by the study of simpler ones; and one of the most remarkable illustrations of this necessary order is afforded by the preparation of dynamical laws by the consideration of statical facts. In statics we consider phenomena in a state of rest, while in dynamics we study their change; and this distinction has been concisely stated by saying that the transition from the statical to the dynamical point of view, consists in superadding the consideration of *time* to that of *space*.

To represent the unknown cause of any change in phenomena, the word *FORCE* has been formed, and is generally retained until the law of that change has been discovered; so that the dynamics of a subject may be said to constitute the explanation of the phenomena belonging to it.

It unfortunately often occurs that names are mistaken for explanations, and people deceive themselves with the belief that, for instance, in attributing chemical decompositions to affinity, attraction, contact-force, catalysis, &c., they explain them.

But owing to the necessary dependence of investigations on our mental operations, there is always a deficiency of facts corresponding to the imperfection of theory;—that is, we only seek and see those facts which are more or less connected with our theoretical notions, and in most cases shut our eyes to such cases as appear contrary to them. This is peculiarly the case with chemical theory and chemical facts at the present day; for our atomic theory represents only certain simple and definite proportions of combination, and our researches have been fruitful in the investigation of such cases alone, the number of compounds of which we know nothing being infinite, compared to those definite ones which we have studied.

In fact, it is certain that if we could sufficiently disengage our minds from preconceived notions on the subject, we should view those substances, which, by more or less troublesome processes, we

separate out from the bodies presented to us by nature, rather as exceptional and artificial products, than as the most normal and natural.

The lecturer submitted that the definite compounds hitherto exclusively acknowledged and studied by chemists, are in truth only exceptionally simple cases of combination, and that the consideration of chemists is only limited to them, because the atomic theory is as yet purely statical. The atomic theory has hitherto been tacitly connected with an unsafe and unjustifiable hypothesis, namely, that the atoms are in a state of rest; the dynamics of chemistry will commence by the rejection of this supposition, and will study the degree and kind of motion which atoms possess, and reduce to this one fact the various phenomena of change, which are now attributed to occult forces. But although it will probably be generally used in connection with the atomic theory, the fact of motion is independent of any particular theory; and however the properties of matter may be conceived, it will remain true, that a change of place among the representatives or possessors of these properties, is constantly going on, which produces the phenomena of chemical combination.

Chemical science has proved the indestructibility of matter, but it has yet to prove the indestructibility of motion or momentum by showing its transfer and dispersion among atoms.

There are many *primæ facie* evidences that time is necessary for chemical action: — but this fact, although it has been noticed, has not as yet entered into the explanation of phenomena.

The one instance in which a certain regular motion of the constituents of a mixture was first proved, is the process of etherification, of which the anomalous character has long since attracted the attention and study of many of the most eminent chemists, and has given rise to various theories which respectively represented part of the phenomena.

The lecturer referred to the importance of having a correct standard of comparison for the various chemical groups or molecules, and briefly alluded to the evidence afforded by the formation of the intermediate ethers, that alcohol and various bodies allied to it have of late years been incorrectly represented comparatively to metallic oxides and ethers, and that the weight of alcohol which is truly equivalent to ether or water, is not 46 but 23.

Having proved by a direct experiment that the formation of ether from alcohol is effected by substituting ethyle (C_2H_5) for $\frac{1}{4}$ of the hydrogen of that body, the process of etherification by sulphuric acid was explained by a diagram, on which half the hydrogen in sulphuric acid was shown to change places with its analogue ethyle in alcohol; and that the peculiarity of the process, *i. e.* its continuity, is owing to this change of place between hydrogen and ethyle, first taking place in one direction and then in the opposite; that is, that sulphuric acid becomes sulphovinic acid by taking up ethyle instead of an atom of hydrogen, and that it is then re-converted into sulphuric

acid by resuming hydrogen instead of this ethyle, the first change forming water, the second ether.

By using successively two different alcohols, it was shown that the two steps of this decomposition can be separated and their reality proved. The process of etherification is thus effected by a succession of double decompositions, each of which considered individually is perfectly conformable to the law of definite proportions; but the alternation and continuous succession so clearly proved in them, is a fact unexplained by that law. A complete analogy between this process and the more familiar cases of chemical action is therefore only to be established by finding in these latter a similar atomic motion.

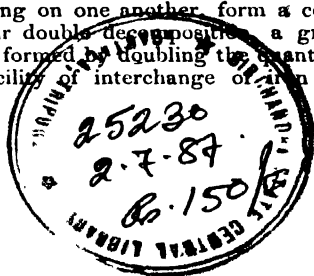
A little reflection is sufficient to show that such a motion actually exists. The fact of diffusion is in reality nothing but a change of place between atoms, effected by the mere action of the particles on one another; and there are many mechanical evidences of the communication of momentum from masses to atoms, and inversely.

It seems perhaps difficult to reconcile the apparent rest of the constituents of a mass with the existence of a continuous atomic motion; but there are many cases in which a rapid and continuous motion produces to our senses the appearance of a phenomenon at rest: thus, the rapid revolution of a white sphere produces the appearance of a circle at rest when seen in front, and that of an ellipse when viewed obliquely.

There are of course many points of view from which the motion of atoms may be considered; but it is inasmuch as it produces or facilitates decomposition, that the chemist has to regard it. We have in etherification an evidence of the tendency of atoms of analogous nature to change places continuously; and it is natural to suppose that the facility of this interchange must be greater in proportion to the analogy between the molecules, and greatest between like molecules. The lecturer expressed a confident hope that he would soon be able to give a direct experimental evidence of this conclusion, and proceeded to show how the admission of it explains, without the supposition of occult forces, the occurrence of double decompositions and the action of masses.

The exchange of analogous particles actually constitutes double decomposition; and its occurrence in alternately opposite directions causes the two substances used to alternate with the two other compounds formed by the exchange of their bases; so that in such a mixture, four substances are constantly to be found, the quantity of each substance corresponding to the average number of atoms which, in each moment of time, are in that state of combination.

Now it is clear that if an equal number of atoms of a hydrogen-salt, and of an iron-salt, reacting on one another, form a certain amount of the products of their double decomposition, a greater number of those products will be formed by doubling the quantity of the hydrogen-salt; for, the facility of interchange of iron with



hydrogen remaining the same, the atoms of the iron-salt will then come more frequently in contact with those of the hydrogen-compound. Thus, on mixing a solution of sesquichloride of iron with sulphocyanide of hydrogen, a deep red colour gave evidence of the interchange of iron and hydrogen, forming sulphocyanide of iron and hydrochloric acid : but this exchange was not an operation effected once for all on the decomposing substances just coming in contact, but is constantly going on in the mixture ; and the quantity of the products of this interchange remains constant, because a similar double decomposition, equal in absolute number of atoms per unit of time, is constantly going on between these products, reproducing the original compounds. In evidence of this, the counterpart of the decomposition of sesquichloride of iron by hydrosulphocyanic acid was shown in the expulsion of this last acid by hydrochloric, proved by the gradual diminution of the red colour on adding hydrochloric acid.

It is well known that caustic soda expels ammonia from its salts. But ammonia also expels soda : for it was shown, that a mixture of ammonia and hydrochlorate of soda dissolves less chloride of silver than the same quantity of ammonia alone ; and consequently, that a saturated solution of chloride of silver in aqueous ammonia is precipitated by dissolving chloride of sodium in it. The same occurs with the ammonio-sulphate of copper.

In conclusion, the lecturer referred to the question of the relative velocity of transfer of analogous atoms in opposite directions, which necessarily determines the proportion of the elements of two salts, contained in the form of their products of double decomposition, on these salts being mixed. On the mixture of equivalent proportions of a couple of salts in aqueous solution, a certain amount of decomposition ensues, forming two other salts, and the chemical force may be considered proportional to the quantity of one couple compared to that of the other. Now as the proportion is only kept up by the number of exchanges in the one direction being ABSOLUTELY the same in each moment of time as those in the opposite direction, it is clear that the relative velocity of interchange must be greatest between the elements of that couple of which the quantity is least ; and chemical force must be inversely proportional to the velocity of these interchanges.

A. W. W.

Friday, June 13.

THE DUKE OF NORTHUMBERLAND, President, in the Chair.

PROFESSOR FARADAY,

On Schönbein's Ozone.

THE object of the speaker was to give a brief account of the present state of this subject ; taking at the same time notice of the ancient facts which belong to it, and the high hopes of progress which it offers for the future. Ozone is produced when the electrical brush passes from a moist wooden point into the atmosphere, and indeed in almost every case of electrical discharge in the air ; or when water is electrolyzed, as in the case of a dilute solution of sulphuric acid or sulphate of zinc ; or when phosphorus acts at common temperatures on a moist portion of the atmosphere. For the latter case take a piece of clean phosphorus about half an inch long which has been recently scraped ; put it into a clean two-quart bottle, at a temperature of about 60° F. with as much water as will half cover the phosphorus ; close the mouth slightly so that if inflammation take place no harm may happen ; and leave it. The formation of Ozone will quickly occur, being indicated by the luminous condition of the phosphorus, and the ascent of a fountain-like column of smoke from it. In less than a minute the test will show Ozone in the air of the bottle, in five or six hours it will be comparatively abundant ; and then the phosphorus being removed and the acids formed at the time washed out, the bottle may be closed and made use of when required for experiments.

The test for Ozone is as follows : 1 part of pure iodide of potassium, 10 parts of starch, and 200 parts of water are to be boiled together for a few moments. A little of this preparation placed on writing paper with a brush being introduced into the Ozone atmosphere is rendered instantly blue from the evolution of iodine :— or if bibulous paper be dipped into this solution, and then dried, it forms Schönbein's Ozonometric test : for a slip being introduced dry into an atmosphere supposed to contain Ozone, after remaining there a longer or shorter time, on being removed and then moistened, instantly becomes more or less deeply blue if Ozone be present.

Ozone when obtained by the three very different processes described is identical in every respect : its properties are as follow : 1. It is a gaseous body of a very peculiar odour : when concentrated the odour approaches to that of chlorine ; when diluted it cannot be distinguished from what is called the electric smell. 2. Atmospheric

air strongly charged with it renders respiration difficult, causes unpleasant sensations, and produces catarrhal effects (by acting powerfully on the mucous membranes). Such air soon kills small animals, as mice, placed in it; so that Ozone in its pure state must be highly deleterious to the animal economy. 3. It is insoluble in water. 4. Like chlorine, bromine, and the metallic peroxides, it is a powerful electromotive substance. 5. It discharges vegetable colours with a chlorine-like energy. 6. It converts phosphorus ultimately into phosphoric acid; it combines with chlorine, bromine, and iodine; it does not unite with nitrogen under ordinary circumstances, but does when lime water is present; and nitrate of lime is formed from which nitre may be readily obtained. 7. At common and even low temperatures it acts powerfully upon most metallic bodies, producing the highest degree of oxidation they are capable of. Lead and even silver is carried at once to the state of peroxides; arsenic and antimony produce arsenic and stibic acids. 8. It transforms many of the lower oxides into peroxides; thus, the hydrate of the oxides of lead, cobalt, nickel, and manganese become in it peroxides: the basic oxide of silver undergoes the same change. 9. It decomposes rapidly the solid and dissolved protosalts of manganese; the hydrated peroxides of the metal being formed, and the acid of the salts evolved. 10. It decomposes the solution of the tribasic acetate of lead; the peroxide of that metal and the ordinary acetate being formed. 11. It rapidly converts the protosalts of iron and tin into persalts. 12. It destroys many hydrogenated gaseous compounds; the combinations of hydrogen with sulphur, selenium, phosphorus, iodine, arsenic, and antimony are thus affected. It appears to unite chemically with olefiant gas in the manner of chlorine. 13. It instantly transforms the sulphurous and nitrous acids into the sulphuric and nitric acids, and the sulphites and nitrites into sulphates and nitrates. 14. It changes many metallic sulphurets (as those of lead and copper) into sulphates. 15. It decomposes many iodides in their solid and dissolved state. By its continued action iodide of potassium becomes converted into iodate of potassa. 16. It changes both the crystallized and dissolved yellow prussiate of potassa into the red salt, potash being evolved. 17. It produces oxidizing effects upon most organic compounds, causing a variety of chemical changes; thus guaiacum is turned blue by it. From the above enumeration it would appear that Ozone is a most ready and powerful oxidizer, and in a great number of cases acts like Thénard's peroxide of hydrogen, or chlorine or bromine.

A number of the actions of this body, such as the bleaching of indigo and litmus, the peroxidation of metals, the conversion of sulphurets into sulphates, &c. were shown, to illustrate the chlorine-like action of the Ozone; and many illustrations supplied by M. Schönbein himself were exhibited.

With respect to the nature of this body, the two chief ideas are — that it is a compound of oxygen analogous to

the peroxide of hydrogen, or that it is oxygen in an allotropic state, *i. e.* with the capability of immediate and ready action impressed upon it. When an ozonized atmosphere is made as dry as possible, and then sent through a red hot tube, the Ozone disappears, being converted apparently into ordinary oxygen, and no water or any other result is produced. This agrees with the known fact, that heat prevents the formation of Ozone, and also with the idea that Ozone is only oxygen in an allotropic state. To show that heat prevents the formation of Ozone a little voltaic battery was associated with a fine platina wire helix, insulated, and connected with the electrical machine; at first the circuit between the battery and the helix was left incomplete; and then on working the machine the brush thrown off from the helix affected the test paper, before described, by the Ozone in it; but when the connexion was complete, so that the helix was ignited, then the electrical brush from it had no power of producing any effect of Ozone.

The speaker described the presence of Ozone in the atmosphere, the mode of testing its presence, and the probable effects it produced there. He referred to Schönbein's recent experiments on the insulation of the oxygen of the air and the peculiar effects produced by this action. He showed by experiments the more recent results of the association of oxygen by light with oil of turpentine and other bodies; and the production of bleaching compounds vying with the hypochlorite of lime in energy. He made it manifest by experiment, that when ether vapour is mixed with air, and a hot platina wire or glass rod introduced, the ether in becoming partially oxidized to produce acid, also produces Ozone, the results bleaching indigo powerfully; and he stated that sulphurous acid, ether, tartaric acid, and many other substances which being first mixed with air or oxygen were then exposed to sunlight, exerted bleaching powers often of a very high degree. The evening concluded with the expression of certain theoretical expectations, or rather possibilities, which were put forth as indicating the probable fertility and importance of the subject, and fitted to excite such philosophers as were engaged in the consideration of the physical qualities of the particles of matter to examine how far the phenomena of Ozone might be carried onward in the illustration and extension of their researches.

Friday, January 23.

SIR JOHN P. BOILEAU, Bart., F.R.S., V.P. in the Chair.

PROFESSOR FARADAY,

On the Lines of Magnetic Force.

THAT beautiful system of power which is made manifest in the magnet, and which appears to be chiefly developed in the two extremities, thence called ordinarily the magnetic poles, is usually rendered evident to us in the case of a particular magnet by the attractive or repulsive effect of these parts on the corresponding parts of another magnet; and these actions have been employed, both to indicate the direction in which the magnetic force is exerted and also the amount of the force at different distances. Thus, if the attraction be referred to, it may be observed either upon another magnet or upon a piece of soft iron; and the law which results, for effects beyond a certain distance, is, that the force is inversely as the square of the distance. When the distances of the acting bodies from each other is small, then this law does not hold, either for the surface of the magnets or for any given point within them.

Mr. Faraday proposes to employ a new method, founded upon a property of the magnetic forces different from that producing attraction or repulsion, for the purpose of ascertaining the direction, intensity, and amount of these forces, not to the displacement of the former method but to be used in conjunction with it; and he thinks it may be highly influential in the further development of the nature of this power, inasmuch as the principle of action, though different, is not less magnetic than attraction and repulsion, not less strict, and the results not less definite.

The term *line of magnetic force* is intended to express simply the direction of the force in any given place, and not any physical idea or notion of the manner in which the force may be there exerted; as by actions at a distance, or pulsations, or waves, or a current, or what not. A line of magnetic force may be defined to be that line which is described by a very small magnetic needle, when it is so moved in either direction corre-

spondent to its length, that the needle is constantly a tangent to the line of motion; or, it is that line along which, if a transverse wire be moved in either direction, there is no tendency to the formation of an electric current in the wire, whilst if moved in any other direction there is such a tendency. The direction of these lines about and between ordinary magnets is easily represented in a general manner by the well known use of iron filings.

The method of recognizing and taking account of these lines of force which is proposed, and was illustrated by experiments during the evening, is to collect and measure the electricity set into motion in the moving transverse wire; a process entirely different in its nature and action to that founded on the use of a magnetic needle. That it may be advantageously employed, excellent conductors are required; and therefore those proceeding from the moving wire to the galvanometer were of copper 0.2 of an inch in thickness, and as short as was convenient. The galvanometer, also, instead of including many hundred convolutions of a long fine wire, consisted only of about 48 or 50 inches of such wire as that described above, disposed in two double coils about the astatic needle: and that used in the careful research contained only 20 inches in length of a copper bar 0.2 of an inch square. These galvanometers shewed effects 30, 40, or 50 times greater than those constructed with fine wire; so abundant is the quantity of electricity produced by the intersections of the lines of magnetic force, though so low in intensity.

The lines of force already described will, if observed by iron filings or a magnetic needle or otherwise, be found to start off from one end of a bar magnet, and after describing curves of different magnitudes through the surrounding space, to return to and set on at the other end of the magnet; and these forces being regular, it is evident that if a ring, a little larger than the magnet, be carried from a distance towards the magnet and over one end until it has arrived at the equatorial part, it will have intersected *once* all the external lines of force of that magnet. Such rings were soldered on to fitly shaped conductors connected with the galvanometer, and the deflections of the needle observed for one, two, or more such motions or intersections of the lines of force: it was stated that when every precaution was taken, and the results at the galvanometer carefully observed, the effect there was sensibly proportionate for small or moderate arcs to the number of times the loop or ring had passed over the pole. In this way, not only could the definite actions of the intersecting wire be observed and established, but also one magnet could be compared to another, wires of different thickness and of different substances could be compared, and also the sections described by the wire in its journey could be varied. When the wire was the same in length, diameter, and substance, no matter what its course was across the lines of force, whether direct or oblique, near to or far from the poles of the magnet, the result was the same.

A compound bar magnet was so fitted up that it could revolve on its axis, and a broad circular copper ring was fixed on it at the middle distance or equator, so as to give a cylindrical exterior at that place. A copper wire being made fast to this ring within, then proceeded to the middle of the magnet, and afterwards along its axis and out at one end. A second wire, touched, by a spring contact, the outside of the copper ring, and was then continued outwards six inches, after which it rose and finally turned over the upper pole towards the first wire, and was attached to a cylinder insulated from but moving round it. This cylinder and the wire passing through it were connected with the galvanometer, so that the circuit was complete; but that circuit had its course down the middle of the magnet, then outwards at the equator and back again on the outside, and whilst always perfect, allowed the magnet to be rotated without the external part of the circuit, or the latter without the magnet, or both together. When the magnet and external wire were revolved together, as one arrangement fixed in its parts, there was no effect at the galvanometer, however long the rotation was continued. When the magnet with the internal wire made four revolutions, as the hand of a watch, the outer conductor being still, the galvanometer needle was deflected 35° or 40° in one direction: when the magnet was still, and the outer wire made four revolutions as the hands of a watch, the galvanometer needle was deflected as much as before in the *contrary direction*: and in the more careful experiments the amount of deflection for four revolutions was precisely the same, whatever the course of the external wire, either close to or far from the pole of the magnet. Thus it was shewn, that when the magnet and the wire revolved in the same direction, contrary currents of electricity, exactly equal to each other, tended to be produced; that those outside resulted from the intersection by the outer wire of the lines of magnetic force external to the magnet; that wherever this intersection was made the result was the same; and that there were corresponding lines of force within the magnet, exactly equal in force or amount to those without, but in the contrary direction. That in fact every line of magnetic force is a closed curve, which in some part of its course, passes through the magnet to which it belongs.

In the foregoing cases the lines of force, belonging as they did to small systems, rapidly varied in intensity according to their distance from the magnet, by what may be called their divergence. The earth, on the contrary, presents us, within the limits of one action at any one time, a field of equal force. The dipping needle indicates the direction or polarity of this force; and if we work in a plane perpendicular to the dip, then the number or amount of the lines of force experimented with will be in proportion to the area which our apparatus may include. Wires were therefore formed into parallelograms, inclosing areas of various extent, as one square foot, or nine square feet, or any other proportion, and being fixed upon axes equidistant from two of the sides could

have these axes adjusted perpendicular to the line of dip and then be revolved. A commutator was employed and associated, both with the galvanometer and the parallelograms, so that the upper part of the revolving wire always sent the current induced in it in the same direction. Here it was found that rotation in one direction gave one electric current; that rotation in the reverse direction gave the contrary current; that the effect at the galvanometer was proportionate to the number of rotations with the same rectangle; that with different sized rectangles of the same wire the effect was proportionate to the area of the rectangle, *i. e.* the number of curves intersected, &c. &c. The vicinity of other magnets to this magnet made no difference in the effect provided they were not moved during the experiments; and in this manner the non-interference of such magnets with that under investigation was fully established.

All these and other results are more fully stated and proved in papers now before the Royal Society. The general conclusions are, that the magnetic lines of force may be easily recognized and taken account of by the moving wire, both as to *direction* and *intensity*, within metals, iron or magnets, as well as in the space around; and that the wire sums up the action of many lines in one result: That the lines of force well represent the *nature, condition, direction, and amount* of the magnetic forces: That the effect is directly as the number of lines of force intersected, whether the intersection be direct or oblique: That in a field of equal force, it is directly as the *velocity*; or as the *length* of the moving wire; or as the *mass* of the wire: That the external power of an unchangeable magnet is *definite* yet illimitable in extent; and that any section of all the lines of force is equal to any other section: That the lines of force within the magnet are equal to those without: and that they are continuous with those without, the lines of force being closed curves.

[M.F.]

Friday, February 13.

THE DUKE OF NORTHUMBERLAND, F.R.S., President,
in the Chair.

W. R. GROVE, Esq., M.A., F.R.S.,

On the Heating Effects of Electricity and Magnetism.

IN the early periods of philosophy when any unusual phenomenon attracted the attention of thinking men it was frequently referred to a preternatural or spiritual cause; thus with regard to the subject about to be discussed, when the attraction of light substances by rubbed amber was first observed, Thales referred it to a soul or spiritual power possessed by the amber.

Passing to the period antecedent to the time of more strict inductive philosophy, viz. the period of the Alchemists, we find many natural phenomena referred to spiritual causes. Paracelsus taught that the Archæus or stomach demon presided over, caused, and regulated the functions of digestion, assimilation, &c.

Van Helmont, who may be considered in many respects the turning point between Alchemy and true chemistry, adopted with some modification the Archæus of Paracelsus and many of the opinions of the Spiritualists, but shewed tendencies of a more correctly inductive character; the term 'Gas' which he introduced, gives evidence of the thought involved in it by its derivation from 'Geist' a ghost or spirit. By regarding it as intermediate between spirit and matter, by separating it from common air and by distinguishing or classifying different sorts of gas he paved the way for a more accurate chemical system.

Shortly after the time of Van Helmont lived Torricelli, who by his discovery of the weight of air was mainly instrumental in changing the character of thought and inducing philosophers to introduce, or at all events to develop the notion of fluids, as agents which effected the more mysterious phenomena of nature, such as light, heat, electricity, and magnetism.

Air being proved analogous in many of its characters to fluids as previously known, the idea of fluids or of an ether was carried on to other unknown agencies appearing to present effects remotely analogous to air or gases.

Sound was included by some in the same category with the other affections of matter, and as late as the close of the last century a paper was written by Lamarck to prove that sound was propagated by the undulations of an ether. Sound is now admitted to be an undulation or motion of ordinary matter, and Mr. Grove considered

that what have been called the imponderables, or imponderable fluids, might be actions of a similar character, and might be viewed as motions of ordinary matter.

Heat was at an early period so viewed, and we find traces of this in the writings of Lord Bacon. Rumford and Davy gave the doctrine a greater development, and Mr. Grove in a communication made by him at an Evening Meeting of this Institution in 1847, shewed that what had hitherto been deemed stumblingblocks in the way of this theory of heat, viz. the phenomena presented by what have been called latent and specific heat, might be more simply explained by the dynamic theory.

In this evening's communication he brought forward some experiments and considerations in favour of the extension of this view to electricity and magnetism, an extension which he had for many years advocated, and which was, in his opinion, supported by many analogies.

The ordinary attractions and repulsions of electrified bodies present no more difficulties when regarded as being produced by a change in the state or relations of the matter affected, than did the attraction of the earth by the sun, or of a leaden ball by the earth; the hypothesis of a fluid is not considered necessary for the latter, and need not be so for the former class of phenomena.

In the cases of heating or ignition of a conjunctive wire or conducting body through which what is called Electricity is transmitted, we have many evidences that the matter itself is affected, and in some cases temporarily, in others, permanently changed; thus if a wire of lead is ignited to fusion by the voltaic battery, the fused lead being kept in a channel to prevent its dispersion, it gradually shortens, and the molecules seem impressed with a force acting transversely to the line of direction of the electricity; at length the lead gathers up in nodules which press on each other as do, to use a familiar illustration, a string of figs.

With Magnetism we have many instances of the molecular change which a ferreous or magnetic substance undergoes when magnetized. If the particles are free to move, as for instance iron filings, they arrange themselves symmetrically. An objection may be made arising from the peculiar form of the iron filings, but Mr. Grove in the year 1845, shewed that the supernatant liquid in which magnetic oxide had been formed, and which contains magnetic particles not mechanically but chemically divided, exhibits when magnetized a change in the arrangement of the molecules, as may be seen by its effect on transmitted light; — a molecular change is also evidenced by the note or sound produced by magnetism, and by other effects.

Assuming that the molecules of iron change their position *inter se* upon magnetization, then by repeated magnetization in opposite directions, something analogous to friction might be produced; and just as a piece of caoutchouc when elongated produces heat, (as it was on this occasion experimentally shewn to do) so a bar of soft iron

might be expected when subjected to rapid changes in its magnetic state, to exhibit thermic effects.

With the aid of the large magnet of the Institution and of a commutator for changing the direction of the Electricity a bar of soft iron was alternately magnetized in opposite directions; and in a few minutes a thermometer placed in an aperture in the iron shewed a rise of temperature of $1,5^{\circ}$ Fahrenheit; the bar being separated from the magnet by flannel, and the magnet being at a notably lower temperature than the bar, this heat could in nowise be attributed to conduction.

The effect of Electricity in the disruptive discharge as in the Voltaic arc and the electric spark, would seem at first sight to offer greater difficulties of explanation on the dynamic theory. The brilliant phenomenal effects of the electric discharge, and the apparent absence of change in the matter affected by it, would at first lead the observer to believe that Electricity was a specific entity.

With ordinary flame or the apparent effects of combustion however, the idea has to a great extent been abandoned that such visual effects are due to specific matter, and it is regarded by many as an intense motion of the particles of the burning body. So with Electricity, if in regard to the disruptive discharge it can be shewn that the matter of the terminals or of the intervening medium is changed, the necessity for the assumption of a fluid or ether ceases, and, to say the least, a possibility of viewing Electricity as a motion or affection of ordinary matter is opened.

To make evident to the audience the relation of the electrical discharge to combustion and the fact that the terminals were themselves affected, the Voltaic arc was taken, first between silver and then between iron terminals; in the first case a brilliant green coloured flame was produced, and in the second a reddish scintillation or spur fire effect, just as in the ordinary combustion of the metals.

So with the discharge of Franklinic Electricity between the same two metals, a strip of silvered leather gave the bright green discharge, while a chain of iron gave the spur fire effect.

The known transport of particles of the terminals from one pole to the other,—the different effects of different intervening media on induction as shewn in Faraday's experiments,—the polar tension of such media, &c. were instances of the train of molecular changes consequent upon electrical action.

Hitherto the polarity of the gaseous medium existing between the metallic or conducting terminals of the electrical circuit was only known as a physical polarity and not shewn to have an analogous chemical character with that existing in electrolytes anterior to electrolysis; but Mr. Grove stated that in a recent communication to the Royal Society he had shewn that mixtures of gases having opposite electrical or chemical relations, such as oxygen and hydrogen, or compound gases such as carbonic oxide, were electro-chemically polarized or had their electro-negative and electro-positive elements

thrown in opposite directions : thus if a silvered plate be made positive in such gases it is oxidized, if negative the dark spot of oxide is reduced ; and an experiment was shewn in which such a plate was thus oxidized and the spot reduced in gaseous media.

Here, as in the other experiments, was an effect on the terminals and an effect of polarization of the intermedium. In the experiments hitherto shewn, solid terminals were used ; it became important to examine what would be the effect of liquid terminals, for instance water ; the spark or disruptive discharge of Franklinic Electricity was readily obtained from its surface, but hitherto no voltaic battery had been found to shew a discharge at any sensible distance from the surface of water.

Mr. Gassiot had procured to be constructed 500 cells of the nitric acid battery, the combination discovered in 1839 by Mr. Grove and first shewn at this Institution in the year 1840. The cells of this battery were all well insulated by glass stems, and as regards intensity of action it was probably far the most powerful ever seen. Mr. Gassiot had kindly lent this apparatus for the illustration of this evening's discourse, and by its aid Mr. Grove was able to shew an experiment which he had first made when experimenting with Mr. Gassiot some time ago, and which produced the effect he had long sought for, viz. a quantitative or voltaic discharge at a sensible distance from the surface of water. The experiment was made as follows :— a platinum plate forming the anode of the battery was immersed in a capsule of distilled water, the temperature of which was raised. A cathode or negative terminal of platinum wire was now made to touch for a moment the surface of the water and immediately withdrawn to a distance of about quarter of an inch ; the discharge took place, the extremity of the platinum wire was fused and the molten platinum attached to the wire but kept up by the peculiar repulsive effect of the discharge was exhibited, as it were, suspended in mid-air, giving an intense light, throwing off scintillations in directions away from the water and only detaching itself from the wire when agitated.

Here water in the vaporous state must be transferred, for the immersed electrode gave off gas, without doubt oxygen, and the molecular action on the negative fused platinum resembled, if it were not identical in character with the currents observed on the surface of mercury when made negative in an electrolyte.

It may be objected to the theory proposed, that electrical effects are obtained in what is called a vacuum, where there is no intermedium to be polarized ; but this objection, though not applicable to the projection of the terminals, could hardly be discussed until experimentalists had gone much further than at present in the production of a vacuum ; the experiments of Davy and others had shewn that we are far off from obtaining any thing like a vacuum where delicate investigations are concerned.

The view of the antient philosophers that Nature abhors a

vacuum which had been much cavilled at, and was supposed to be exploded by the discovery of Torricelli, Mr. Grove thought had been unjustly censured : giving the expression some degree of metaphorical license, it afforded a fine evidence of the extent and accuracy of observation of those who were unacquainted with inductive philosophy as a system, but who necessarily pursued it in practice. Whether a vacuum was possible might be an open question, experimentally it was unknown.

Lastly, in answer to those who might ask, to what practical results do researches such as these lead ? what accession of physical comfort or luxury do they bring ? Mr. Grove took occasion to offer his humble protest against opinions now perhaps too generally prevalent, that science was to be viewed only or mainly in its utilitarian or practical bearings. Even regarding it in this aspect, were it not for the devotion which the love of knowledge, which the yearning anxiety to penetrate into the mysteries of our being and of surrounding existences induced ; the practical results of science would not have been attained ; the band of Martyrs to Science from Socrates to Galileo would not have thought and suffered without a higher incentive than the acquisition of utilitarian results : without disparaging these results, indeed regarding them as necessary consequences of any advance in scientific knowledge, he considered that the love of truth and knowledge for themselves was the great animating principle of those who rightly pursued science ; that, based upon an enduring quality of our common nature, this feeling was rooted in far firmer foundations, that it led to greater and more self sacrificing exertions, than any capable of being induced by the hopes of augmenting social acquisitions, and was an attribute and an evidence of the non-transient part of our being.

[W. R. G.]

Friday, February 27.

THE DUKE OF NORTHUMBERLAND, F.R.S. President,
in the Chair.

DR. LYON PLAYFAIR, C.B., F.R.S.

On three important Chemical Discoveries from the Exhibition of 1851 — A. Mercer's Contraction of Cotton by Alkalies; — B. Young's Paraffine and Mineral Oil from Coal; — C. Schrötter's Amorphous Phosphorus.

[The following statements and arguments were supplied by DR. LYON PLAYFAIR as embodying considerations which he desired to impress on the attention of the Members of the Royal Institution.]

It is incumbent on those who, like myself, have been connected with the Great Exhibition, to inculcate its teachings in order that it may influence the future, by being a starting point for industry. Unless it imparts new life to productive industry, it has failed in the attainment of its object, and will, in history, degenerate into the record of a gigantic show, fitted only to pander to an idle curiosity. All of us have, no doubt, examined it with a higher object, and have derived lessons varying in character and amount according to the opportunities which we enjoyed in their acquisition. Those who have attended to its teachings with regard to the comparative progress of manufactures in different countries, owe it as a public duty to announce their convictions on a subject of such large social importance.

My official connection with the Exhibition has enabled me to give more attention to it than most of those whom I have the honour to address, and convictions unfavourable to our position, as an industrial nation, have impressed themselves with such force upon my mind, that you will not be surprised that I seize every opportunity of directing public attention to them. I have already done so in a formal manner, on two previous occasions, and I rather depart from the custom of bringing before you subjects of original research at these evening meetings, in order that I may advocate the necessity of a more intimate union between science and practice in this country, at an Institution, whose proudest boast it is to have largely advanced the discovery of abstract truths, while it has always encouraged, at the same time, their applications to the increase of human resources and enjoyments.

In this lecture, however, I shall rather urge this point as a natural

consequence of the subjects chosen for illustration of my argument than by any doctrinal exhortations, because these are not needed to strengthen your general convictions.

Our nation has acquired a proud position among the industrial states of the world, partly by the discoveries of her philosophers, partly by the practical powers and common sense of her population, but chiefly by the abundance and richness of her natural resources. Our fuel is abundant and cheap, and our iron and the lime necessary for its production are associated with it, so that all three may be extracted together under the most favourable circumstances. These local advantages gave to our country enormous powers of production, and, under the favouring influences of an accidental combination, it supplied its produce to the rest of the world. Circumstances remaining the same, our industrial position was secured, and we have been thus lulled into a fatal apathy; for conditions were in fact varying with great rapidity, and the world at large was passing through a state of remarkable transition.

Setting aside the questions of capital and labour, which are not adapted for discussion in this place, the progress of manufactures is made up of two factors, possessing very different values. One of these represents the raw produce,—the other, the intellect or science employed to adapt it to human wants. As civilization advances, the value of the raw material as an element of manufactures diminishes, while that of the intellectual element is much enhanced. Improvements in locomotion by sea and land spread over the world the raw material formerly confined to one locality; and a time arrived when a competition of industry became a competition, not of local advantages, but of intellect.

It was obvious that when improved locomotion gave to all countries raw material at slight differences of cost, that any superiority in the intellectual element would more than balance the difference. The Continental States, acting on a perception of this truth, saw that they could only compete with English industry by instructing their populations in the principles of science. Hence have arisen, in their capitals, in their towns, and even in their villages, institutions for affording a systematic training in science; and industry has been raised from the rank of an empirical art to that of a learned profession. The result is seen in the fact that we now meet most European nations as competitors in all the markets of the world. The result is palpably forced upon us by our actual displacement from markets in which we had a practical monopoly. The result was obvious in the Exhibition, where we saw many nations, formerly unknown as producers, frequently approaching, and often excelling us in manufactures, our own by hereditary and traditional right.

The teaching of the Exhibition was to impress me with the strongest conviction that England, by relying too much on her local advantages, was rapidly losing her former proud position among manufacturing nations; and that unless she speedily adopted measures to cultivate the intellectual element of production, by instructing her population in the scientific principles of the arts which they

profess, she must inevitably and with rapidity lose those sources of power, which, in spite of the smallness of her home territory, have given to her so exalted a rank among nations.

With these convictions you will not be surprised that I have chosen subjects connected with the Exhibition, although I have no merit or part whatever in their discovery. I have selected them for the following reasons.

We have a great reliance on the practical sagacity or common sense of our population,—certainly superior to that of any part of Europe: but we have not strengthened it by communicating scientific knowledge to those who are entrusted with the exercise of this practical power; and, hence, this common sense, unaided by the rules of science, has gradually assumed a sway over our manufactures. In other words, conjectural judgments have usurped the place of systematic knowledge. Practice and science have been followed out separately, as having no immediate connection. This separation, and even practical antagonism, has been fatal to our progress in industry; for manufacturers, as a body, have ceased to perceive that abstract science forms the roots of the tree of industry, and that to separate them is to sever the tree from its roots. In order to restore vigour to our declining industry, it is essential that confidence in the powers of science should be imparted to practice, and that the latter should be taught that it is, even as a question of social policy, highly important to encourage discoveries in abstract truths, however apparently remote from practice; because science only benefits industry by its overflowings, arising from the very fulness of its measure.

Every abstract truth, in its due time, adds to human resources and enjoyments, and it is this text that I wish to inculcate from examples derived from the Exhibition. One of the last generalizations of the great Berzelius, was that of *allotropism*, a name only eleven years old, and fully explained by him only six years since; and yet this generalization, apparently, at the time, only of abstract interest, entirely remote from practical application, produced as fruit the three most original, and, I think, the most important, practical discoveries of the Exhibition.

Having thus introduced the subject of his Lecture, Dr. Playfair proceeded to offer certain examples of *allotropism*. It had long been known that bodies crystallized in two or more incompatible forms. Thus, carbonate of lime as arragonite crystallizes in prisms; whereas as calcareous spar it crystallizes in rhombs. Sulphur also crystallizes in two incompatible forms; so does the garnet. This is termed *dimorphism*. When two such forms exist they are found to be maintained in unequal stability; it appears, in fact, as if one form was normal, and the other forced or strained. Thus a prism of arragonite is subject to change into rhombs of calc spar; and sulphur crystallized by heat in oblique rhombic prisms passes in a few days into a mass of rhombic octohedrons. Not only may the chemical and physical characteristics of such dimorphous bodies

differ, but their colour and their specific gravity. Thus, the sulphuret of iron (Fe. S_2), when crystallized in cubes, is persistent in the air; but when occurring in a rhombic form, readily passes into cop-peras or sulphate of iron.

Applying the preceding remarks to non-crystallized bodies, it was equally found that many were susceptible of allotropic modification. Thus cinnabar and vermilion were of precisely similar chemical composition with the black sulphuret of mercury. Again, the sesqui-sulphuret of antimony might be black or orange. Iodide of mercury is commonly red; when heated, however, it passes into a yellow powder, which by simple pressure and rubbing with a hard body becomes red again. Sugar is a remarkable instance of a solid capable of assuming two allotropic states; as sugar candy it is crystallized, as barley sugar it is amorphous; yet the composition of sugar in either case is the same. Nor are liquids exempt from the strange state of allotropism, — sometimes indeed manifesting a condition even beyond allotropism (*isomerism*), and not allowing us to reconvert them to their primitive state. Thus the chemical composition of oil of turpentine, of rosemary, of lemons, of copaiba, are identical, yet no one of these bodies has hitherto been turned into the other. The steroptane of otto of roses is identical in composition with coal gas, yet chemists are unable to change one into the other. The term *isomerism* has been commonly employed in relation to bodies of like atomic composition, and has reference to equality of parts. The term *allotropism* is a better denomination, and has reference to the condition of unlike properties.

The preceding remarks by Dr. Playfair were introductory to an exposition of the respective discoveries of Mr. Mercer, Mr. Young, and Dr. Schrotter.

A.—Mercer's process consists in bringing cotton fabrics in contact with a solution of soda (cold), or a solution of dilute sulphuric acid, by subjecting it to either of which processes cotton acquires certain remarkable properties. In the first place, the texture becomes very much corrugated, and hence proportionably finer; it also assumes acid properties, rendering it more capable of taking up dyes. The process of induction which led Mr. Mercer to his final discovery was curious. He started from the point of investigating the laws which determined the flow of water at various temperatures through minute tubes. From water he proceeded to aqueous saline solutions; from tubes he proceeded to their equivalent, namely, closely-folded woven tissue. Selecting for this purpose a thick reduplication of calico, fold on fold, and employing an aqueous solution of soda, Mr. Mercer found that, by passing the solution through the calico, soda was removed. This removal he attributed to the act of filtration; but, subsequently finding that mere immersion of the calico in the same solution effected a like result, he concluded that the result was due to an actual combination of the cotton with the soda — a *calico-ate* of soda (if the lecturer might be permitted that form of expression) was generated.

The result of this agency of soda was, as formerly remarked, a physical corrugation, and an acquisition of certain chemical qualities. The former change was evident to the eye. Dr. Playfair exhibited two stockings, one of which being nearly double the size of the other, although both came equal in size from the loom. The difference had been occasioned solely by chemical not mechanical agency. Dr. Playfair, in developing the numerous practical applications of this physical effect, showed that, besides the most obvious one of producing a material of increased fineness, the cotton thus prepared was far more capable of being dyed. Hot soda solution would not answer; and this fact was remarkable and had its analogue in those salts which deposited themselves anhydrous on boiling. Instead of soda sulphuric acid might be employed; in which case it formed, in combination with the cotton fibre, an easily decomposable conjugate acid.

B.—Some years ago Liebig stated that one of the greatest discoveries of chemistry would consist in converting coal-gas into a solid form, thus enabling it to be burned like a candle. This had, in a manner, been accomplished by Mr. Young. About three years since, Dr. Playfair drew the attention of Mr. Young to a spring of mineral oil, containing paraffine, and occurring in a coal-mine in Derbyshire. The liquid had been extensively applied by Mr. Young as a lubricating agent; a use which Reichenbach had long ago suggested. After a period, however, this spring ceased to flow, when Mr. Young applied himself to an investigation of the theoretical conditions under which it might be artificially formed. This gentleman saw that it would be difficult to convert gas into an allotropic form, whereas it was evident that gas must first come from a solid; hence he hoped to succeed in procuring the body before it assumed its gaseous state.

The illuminating portion of coal gas consists chiefly of olefiant gas, and the latter is isomeric with solid paraffine. But the allotropism does not end here; the peculiar slow distillation of coals yielding solid paraffine, also yielded another isomeric or allotropic compound in the form of a lubricating oil, besides the additional products of a burning oil, and naphtha.

Dr. Playfair now explained, by the aid of a diagram, the slow distillation process of Mr. Young, employed in generating his allotropic form of olefiant gas, and directed the attention of his audience to some candles made of coal paraffine on the lecture table.

C.—Schrötter's process of manufacturing amorphous or allotropic phosphorus was the third in Dr. Playfair's series. The properties of phosphorus in its ordinary condition are well known. It is spontaneously inflammable and highly poisonous; whereas the amorphous or allotropic phosphorus is neither spontaneously inflammable nor poisonous. Hence its great use in the manufacture of lucifer and congrave matches; an operation which not only imperilled the premises wherein it is conducted, but also the lives of those conducting it, causing the most frightful and fatal disease of the jaws and facial bones.

Common phosphorus, when heated to about 460 or 480, changes

into the allotropic condition, but a slight increment of heat changes it back again. Hence the manufacture of this substance on a large scale is attended with difficulties which Dr. Playfair had no doubt would be eventually overcome by the energy of Mr. Sturge the patentee. The specific gravity of ordinary phosphorus is 1.77 — of amorphous phosphorus, 1.964. Common phosphorus is soluble in bi-sulphuret of carbon, whereas the amorphous variety is not. Common phosphorus bursts into flame when brought into contact with iodine, whereas the amorphous or allotropic variety does not. Common phosphorus is luminous at very low temperatures, whereas the amorphous variety only commences to be luminous at a temperature of 500° F. In forming lucifer matches by means of allotropic phosphorus, there is experienced the difficulty that it does not ignite by friction ; hence it has to be mixed either with chlorate of potash, oxide of lead, or sulphuret of antimony, when friction takes effect and generates flame.

Having thus discussed the experimental portion of his lecture, Dr. Playfair concluded as follows : —

These three practical discoveries, for I think they are entitled to be considered as such, and not merely as inventions, have emanated from men all highly educated in chemical science. It is a proud subject of praise and of congratulation, that the two first discoverers, Mr. Mercer and Mr. Young, have, by the aid of science, raised themselves from the position of working artizans to that of employers in works involving considerable capital in their prosecution. Science has been to them a true power, the more so as in the arts which they profess, the manufacturers have usually been men of technical and not of scientific knowledge. The very fact of their success is a convincing evidence of what an immense development our industry might receive, if its sons were able to take advantage of the knowledge which science is constantly showering down upon the world.

There is a wide chasm between the laboratory of the philosopher and the workshop of the manufacturer — a chasm which must be bridged over by those who understand the nature of the foundations on either side. In general it is not the duty of the philosopher to do this ; it is more important for social progress, that he should continue to benefit the world by new accessions of truth, leaving to others to apply them to the promotion of the comforts and happiness of the human race. If technical men become disciples of science, then their acquaintance with the wants and requirements of manufacturers would enable them to derive from its teachings the knowledge requisite to apply it to the desired ends. Science should roll on, as it does now, a mighty river, from the abundant waters of which streams may be derived to fertilize the lands over which they pass ; for in the course of nature, these overflowings are restored in the form of refreshing showers. Their beneficial effects will however depend upon the skill of those who

construct the channels destined to direct the waters for the uses of industry.

It is no new truth that science should always be ready to benefit industry by instructing those engaged in it, rather than by directly uniting with it. This truth is as old as the mythology, where we find no celestial so beneficent to industrial arts as Minerva, although she always preserved an independent existence, notwithstanding the passionate wooing of Vulcan, the god of industry. We have had it inculcated by the sages of all countries, strongly enforced by our own Bacon, and eloquently advocated in the theatre of this Institution by Davy.

Abstract science could not, if it would, cause itself and industry to progress satisfactorily by means of its discovering philosophers. There must be other means of conveying its God-born truths to industry, in order to freshen and invigorate its existence. The results of continental success indicate that the true way for those requiring aid is to come to the fountain of knowledge and take that which they need.

I need not detain you longer on this subject, except again to urge you to consider what must be the result of the system of instruction pursued abroad. In addition to many provincial schools, France has two central colleges of arts and manufactures, in one of which 300 of the best youth of France commence their education in science just where our colleges leave off, and after two years, they are poured into the provinces to impart to industry the principles of science which they have there attained. Prussia, Austria, Russia, and the Northern States are encouraging the same kind of education, and even yet more extensively. Need we be surprised, then, that they are progressing so rapidly in manufactures, in spite of their dear fuel and machinery. Recollect that we *have* reached that state when in future the *competition of industry must be a competition of intellect.*

Is England in a prepared state to meet this intellectual competition? Have we adapted the system of instruction in our schools to the wants and necessities of the age? Has science, or a knowledge of God's works and God's goodness and wisdom, yet become an important part of the instruction of our sons of industry, or, do we not, by an antiquated notion, preserve the idea that the classical learning of the thirteenth century is all-sufficient for the requirements of the nineteenth century?

These questions are truly important if we desire to see England keep her ground in the industrial struggle of nations. I wish not to underrate any branch of human learning; but I do vehemently desire to see banished from our schools the bed of Procrustes, to the dimensions of which our children are clipped or extended until they are so changed in their natural aspirations for science, that it is very difficult, in after life, to communicate that amount which is necessary for its application to industry. I need not say, therefore, that until scientific instruction be added to the general system of education of our youth, that England cannot expect to be foremost in the industrial race of nations.

Already we see our capital largely employed to import foreign talent into our manufactures, and by this, in many cases, we retain our superiority. But it does not require much acumen to perceive the wretchedness of this policy as regards the nation, which, careless of the education of her own sons, sends her capital as a premium to the advancement of that intellectual knowledge in foreign states, who use it as the means of her destruction.

Excuse me if I have expressed my convictions on these points more strongly than you feel them; but they have taken such strong hold on my mind, that I cannot see safety for the future of our nation unless by a great and comprehensive improvement in the instruction of her people. I shall conclude in the language of Davy, when he addressed you on the benefits conferred by this Institution both on science and on industry:—

“There is no country which ought so much to glory in the progress of science as this happy island. Science has been a prime cause of creating for us the inexhaustible wealth of manufactures; and it is by science that it must be preserved and extended. We are interested as a commercial people; we are interested as a free people. The age of glory of a nation is also its age of security. The same dignified feeling which urges men to gain dominion over nations, will preserve them from the dominion of slavery. Natural, and moral, and religious knowledge are of one family; and happy is the country, and great its strength, where they dwell together in union.”

[L. P.]

Friday, April 23.

W. POLE, Esq. M.A., F.R.S., Treasurer and Vice-President,
in the Chair.

THE REV. BADEN POWELL, M.A., F.R.S., &c.

SAVILIAN PROFESSOR OF GEOMETRY, OXFORD,

On the Analogies of Light and Heat.

THE researches of Sir W. Herschel, Sir J. Leslie, M. De La Roche, and others, long since established the existence of well marked differences in character, not only between the radiation from the Sun and that from terrestrial sources, but even among these latter, according as the source was luminous or not: and this especially as regarded its transmissibility through various screens and the absorptive effect of different surfaces.

But the most striking peculiarity in the radiation from flame was established by Sir W. Herschel and afterwards extended to gas-lights by Mr. Brande, in that even at considerable *distances*, after passing through a *thick glass lens*, without heating it, the concentrated rays produced *heat on a blackened thermometer* at the focus, exactly as in the case of the solar rays.

This pointed to a peculiar distinction (also recognized by Sir J. Leslie) and shewed that *the mere proportion* of heat transmitted by a screen (as in De la Roche's experiments) was not the essential characteristic, but that further distinction as to the *specific nature* of the rays, was wanted. This want it was attempted in some measure to supply in some experiments by the author of this paper, (Phil. Trans. 1825) in which the *character of the different rays as to TRANSMISSIBILITY* through screens was examined IN COMBINATION *with the conditions of the ABSORBING SURFACE*.

This last is a point even yet little understood; but thus much is clear:—

(1) A certain peculiarity of *texture* in the external lamina is favourable to the absorption of radiant heat, probably in all cases.

(2) *Darkness of colour* is peculiarly favourable to the effect for *the Sun's rays*, and wholly overrules the first condition.

In *terrestrial luminous hot bodies* it does so to an extent sufficient to give very marked indications. But this (as the author shewed, in the experiments referred to), applies to *that portion only* of the compound rays, which is also *transmissible* through glass, the non-transmissible portion is subject wholly to the former condition, as are *all* the rays from *non-luminous* sources (as was shewn by Leslie and others).

Hence the distinction of *at least two species* of heating rays emanating at the same time from the same *luminous* source.

From the neglect of this distinction much confusion has been kept up: and statements involving such confusion have been repeated from one elementary treatise to another.

Again; notwithstanding that the experiments of Leslie and others on the *absorption* of heat from *non-luminous* sources, as well as those of Professor Bache on the *radiation from surfaces*, demonstrate that the effect has *no relation* whatever to *colour*, yet the contrary assertion has been often persisted in.

Again "dark heat" is often spoken of without recollecting that rays of the very same quality and properties exist in the compound radiation from *luminous* sources.

The conclusions drawn from later experiments, (performed with all the advantages derived from the beautiful invention of the thermo-electric instrument of Nobili,) in many instances, are still vague, from want of attention to the distinction of *different species of heat* emanating at the same time from the same source.

Melloni, in a most extensive and valuable series of experiments, taking as the sources of heat successively flame, incandescent metal, boiling mercury, and boiling water, and applying in each instance a long series of substances as screens, estimated the proportion of rays out of 100 stopped, which was very different for each screen and each source: evincing wide differences in "*diathermanity*," while *rock salt* alone was almost totally "*diathermanous*" to rays from all sources alike.

But we must still ask, what *species* of rays were those respectively stopped and transmitted? To take the *per centage* simply is ambiguous; the body of rays is not homogeneous; the property of transmissibility should be viewed in combination with other properties of the specific rays, such as those evinced in their relations to the texture or colour of the absorbing surface.

Nor is the ambiguity removed, though the difference of *source* is specially referred to, if the heterogeneity of rays from the *same* source be overlooked. The mere classification of sources into *luminous* and *non-luminous* will not suffice: still less a reference to their *temperatures*: it being perfectly well known that the *temperature of luminosity* is very different for different substances.*

Again Melloni has shewn that the *diathermanity* is not proportional to *transparency*, by a classified series of transparent screens with the *lamp*.

It must however be recollected that the term "*diathermanity*" is applied indiscriminately to a heterogeneous body of rays: out of which *some species of rays* are entirely stopped, others entirely transmitted; and the great differences in "*diathermanity*" for heat

* References in detail to all the different researches here mentioned, will be found in the Author's Two Reports on the state of our knowledge of Radiant Heat in the British Association Reports, 1832 and 1840.

from different sources, which Melloni has also established, are nothing else than *absorption of PECULIAR rays* by each medium, not more anomalous than the corresponding absorptions of *luminous rays* by different transparent media so little as yet reduced to law.

While *rock salt* is analogous to colourless media for light, *alum* on the other hand is totally impermeable by heat from dark sources, and partially so by rays from the lamp; that is, wholly impermeable for that portion of the rays which are of the *same kind* as those from non-luminous sources, and permeable to the others.

By other sets of experiments Melloni shewed that rays from the lamp transmitted in different proportions by various screens and then equalized, were afterwards transmitted by *alum* in equally various proportions: or as he expresses it "possess the diathermancy peculiar to the substances through which they had passed."

But this implies no new property communicated to the rays. It shews that as *different specific rays* out of the compound beam were transmitted in each case by the first screen, *alum*, though impervious to the lower heating rays, is permeable by these higher rays; and in different degrees according to their *nature*; an effect simply dependent on the heterogeneity of the compound rays from a lamp.

Again with differently coloured glasses peculiar differences of diathermancy were exhibited with rays from a lamp, incandescent metal, and the sun: but not more various or anomalous than the absorption of specific rays of light.

And besides considerations of this kind it must always be borne in mind that a *blackened surface* (like that which was used in all these experiments) itself is *unequally absorptive for the different rays*.

The solar heat being freely transmissible through all colourless transparent media along with the light, there would be no peculiar advantage in experimenting on the solar spectrum formed by a rock-salt prism. Melloni however with such a prism on interposing a thick screen of water, found the most heating rays (*i. e.* those at or beyond the red end) intercepted, as they are known to be by water: and this caused the position of the *relative* maximum to be apparently shifted higher up in the spectrum, even to the position of the green ray.

On the other hand many coloured glasses, he found, absorbed the rays in various proportions, yet they left the point of maximum heat unaltered: *i. e.* though variously absorptive for the higher rays, they were not of a nature to stop the lower, or most heating rays.

One result indeed is recorded which seems at variance with all other experiments on the solar rays: a peculiar green glass (tinged by oxide of copper) was found to absorb so entirely all the most heating rays that the remaining portion produced no heat, though when concentrated by a lens they gave a brilliant focus. Speaking generally however, these experiments only confirm what is on all hands admitted, *viz.* that the *illuminating* and *heating* powers follow very different laws, with relation to the different rays.

The grand discovery by Melloni of the true REFRACTION OF HEAT even of that kind which constitutes the whole radiation from dark sources, by means of the *rock salt* lens and prism, and its extension by Professor Forbes to the determination of the *index of refraction* (μ) for the most heating rays from all sources both luminous and non-luminous, gave the first actual proof of the real analogy, of the propagation of heat by waves in an ethereal medium: which was further carried out when it was shewn from Cauchy's theory that for different wave lengths (λ) there must be in every medium a certain *limit of all refrangibility*. that is, as we suppose (λ) to increase, large changes in (λ) will give continually smaller changes in (μ), and when (λ) is very great compared with (Δx) the intervals of the molecules, then the index (μ) assumes its limiting value which is not greatly below that for the extreme red ray, and with this, the index for the lowest heat coincides.

This is seen directly from the formula *

$$\frac{1}{\mu^2} = P - Q \left(\frac{\Delta x}{\lambda} \right)^2 + R \left(\frac{\Delta x}{\lambda} \right)^4 - \&c., \text{ which when we suppose } \left(\frac{\Delta x}{\lambda} \right) = 0 \text{ will have for its limiting value } \left(\frac{1}{\mu} \right) = \sqrt{P}$$

The results from observation for Rock salt compared with this theory are as follows :

Rock Salt.

rays	μ	
	obs.	theory
mean light	1.558	. .
red ray	1.540	. .
$\lambda = .000079$. .	1.529
dark hot metal	1.528	. .
Limit	. .	1.527

But it is to the capital fact established by Professor Forbes, of the *polarization* of heat from *dark* sources (for with *luminous* sources little doubt could exist), with all its remarkable train of consequences, that the complete analogy with light is seen in the most uninterrupted point of view; — the transverse vibrations, the dipolarization, the consequent interferences, the production of circular and elliptic vibrations under the proper conditions. — to those familiar with the

* See the Author's Treatise "On the Undulatory Theory applied to the Dispersion of Light," &c. London, J. W. Parker, 1841, pp. 71-122.

wave-theory present an irresistible accumulation of proof of the identity of the rays of heat with a succession of waves in an ethereal medium: exhibiting different properties in *some* dependence on their wave-lengths.

Among the most recent researches on the subject are those of Mr. Knoblauch (of which a translation is given in Taylor's Foreign Scientific Memoirs, Part xviii. and xix.) and they are not to be surpassed for extent and accuracy of detail.

One series is devoted to the examination of the alleged differences in radiation of heat *proportioned to the temperature* of the source. This as before observed is an untenable hypothesis, but Mr. Knoblauch distinctly refutes it by a series of experiments on alcohol flame, red hot metal, hydrogen flame and an argand lamp, whose *temperatures* are in the order of enumeration beginning with the highest: but the power of their heat to penetrate screens is found to follow exactly the reverse order. And even with lower stages of heat, the effects bear no proportion to the *temperatures* as such. Hence the effect is evidently not due to a mere extrication of the heat of temperature, but is of a peculiar kind. In a word, agreeably to the preceding remarks, the different species of rays, more or less compounded together in the several cases, exhibit their diversities of character in developing heat by their absorption. One very peculiar result is, that Platinum, at a stage intermediate between red and white heat, transmits through all the screens employed rather less heat than when at a red heat. That is, these intermediate rays are of such a wave-length as to be subject to a peculiar absorption by these screens: while at the same time possibly less of the former may be emitted.

In another section Mr. Knoblauch adverts to the effects of surfaces on the absorption of rays, and particularly remarks (p. 205); "The experiments of B. Powell and Melloni have shewn that one " and the same body is not uniformly heated by rays from different " sources, which exert the same direct action on a blackened thermoscope;" a statement which does not very intelligibly express any conclusion of the author's. Mr. Knoblauch however supports it by elaborate experiments shewing, as might be anticipated, that an argand lamp affects a surface of carmine less, and one of black paper more, while a cylinder heated to 212° affects the carmine more and the black paper less.

Another extensive series, on the effect of surfaces on radiation, is directed to shew that the effect is independent of the source whence the heat so radiated, was originally obtained.

Among the very multifarious results referring to screens and surfaces obtained by Mr. Knoblauch, it can here only be remarked that none of those varied facts appear to present anything *at variance* with the principles here advocated, while in the general conclusions which he indicates at the close of his memoir, the author though professedly avoiding all hypothesis, yet distinctly intimates his conviction of the heterogeneity of the heating rays increasing as the condition of the source rises in the scale from a low heat up to lumi-

nosity or combustion : and that the diversities of heating effect on different media, are due to a selective absorption of particular species of rays, from peculiarities in the nature of those substances, and analogous to the absorption of particular rays of light by coloured media.

It must not however be omitted to notice, however briefly, another recent set of researches of high interest, those of M. Silberman ; in which (among others) the very remarkable fact is established, that on transmitting a narrow ray of heat from a heated wire, through *rock crystal*, there is a singular difference according as the ray passes *parallel* or *perpendicular* to the axis of the crystal : the effect being indicated by having the further side of the crystal coated with a fine composition of wax, the portion of which in the direction of the ray is melted in a *circular* form in the first instance and in an *elliptical* in the second.

The general fact of the heterogeneity of heating rays, especially from luminous sources, is fully recognized by Melloni as in some sense the conclusion from all his experiments.

The hypothesis that this heterogeneity consists simply in differences of wave-length would seem a probable one ; though it is still possible, as Professor Forbes suggests, that some other element may also enter into the conditions.

This view has been extended by M. Ampère so as to refer both luminous and heating effects to the *same* rays : — a view controverted by Melloni, chiefly on the ground, evinced by several classes of experiments, that *the intensity of the heating effect* (especially in the solar rays) *follows no proportion to that of illumination* ; an argument which really amounts to little unless the theory obliged us to infer that the amount of illumination must follow the *same law* as that of heat ; which it manifestly does not ; since the nature of the effect in the one case is wholly dependent on the unknown constitution of the optic nerve ; according to which some precise proportion of the impinging vibrations, with a particular wave-length, is that which gives the greatest perfection of *vision* : while for *heat* the effect has no reference to such peculiar conditions, but is dependent in some way on longer wave-lengths, and probably more simply connected with the intensity or amplitude of the vibrations.

On this theory our view of the case would be thus : —

A body heated below luminosity begins to give out rays of large wave-length only. As it increases in luminosity it continues to send out these, and at the same time others of diminishing wave-lengths, till at the highest stage of luminosity it gives out rays of all wave-lengths from those of the limit greater than the red end of the spectrum, to those of the violet end, or possibly less.

Rays of all these species are transmissible and refrangible by Rock salt ; and many of them with numerous specific distinctions by other media.

They are all *more* or *less* capable of exciting *heat* when absorbed or *stopped*: though in some the effect is perhaps insensible. Both this property and that of their transmissibility seems to depend in some way on the *wave-length*, though in no simple ratio to it.

The absorptive effect due to *texture* of surfaces has some *direct* relation to the magnitude of the wave-length, especially near the limit. While that due to *darkness of colour* is connected with shorter wave-lengths such as belong to rays within the limits of the *light spectrum*: and in any case when a ray impinges on any absorbing substance, its vibrations, being stopped, communicate to the molecules of the body vibratory movements of such a kind as constitute heat of temperature.

The peculiar molecular constitution of bodies which determines their permeability or impermeability to rays of any species, gives rise to all the diversities of effect, whether luminous or calorific. We thus escape all such crude ideas, at once difficult and unphilosophical, as those either of two distinct material emanations producing respectively heat and light, or of a conversion of one into the other; and obtain a view far more simple and consistent with all analogy.

[B. P.]

Wednesday, March 31.

SIR CHARLES LEMON, BART, M.P., F.R.S.
in the Chair.

M. P.-H. BOUTIGNY (d'Evreux).

Etudes sur les Corps à l'état sphéroïdal.

LES PHÉNOMÈNES qui se rapportent à cette partie de la Physique ont du être entrevus dès la plus haute antiquité. Le premier qui fit chauffer un silex, un morceau de granit ou un métal quelconque, et qui laissa tomber accidentellement ou volontairement sur sa surface quelques gouttes d'eau dut remarquer que cette eau ne se comportait pas comme lorsqu'on l'échauffait dans les conditions ordinaires. En remontant aux traditions les plus reculées, peut-être trouvait-on la trace de cette observation dans ces paroles du Livre de *La Sagesse* (verset 19 du chap. xix) : "Le feu surpassant sa propre nature, brûlait au milieu de l'eau, et l'eau oubliant la sienne ne "l'éteignait point." Au moyen âge, les verriers paraissent avoir connu cette propriété et en avoir fait une application assez ingénieuse à leur art. Quelle que soit l'antiquité de cette remarque, on peut dire que les phénomènes qui s'y rapportent n'ont été réellement observés que vers le milieu du dernier siècle, et à peu près dans le même tems par Eller* et par Leidenfrost.† Depuis lors le petit nombre de physiciens qui se sont occupés de ce phénomène, n'ont presque rien ajouté ni aux expériences qui l'eussent reproduit sous de nouvelles formes, ni à son explication théorique.

Tout le monde a observé que lorsqu'on laisse tomber quelques gouttes d'eau dans une capsule rougie au feu, cette eau, loin de se répandre sur le métal et de le mouiller, prend la forme de globules qui roulent à sa surface sans y adhérer. Voilà l'expérience vulgaire qui est le point de départ de toutes les recherches de cet ordre. Dans les cours de Physique, on s'était contenté de dire que l'eau,

* Histoire de l'Académie de Berlin, 1746, page 42.

† De aquæ communis qualitativus. Duisbourg, 1796.

mise en contact avec un corps incandescent, n'adhérât point à sa surface et s'évaporerait plus lentement que lorsque le même corps était porté seulement à la température où le liquide entrait en ébullition ; mais on n'avait donné de ce phénomène aucune explication satisfaisante.

Lorsqu'on projette quelques gouttes d'eau sur une plaque d'argent légèrement concave, à la température ordinaire, le liquide en mouille la surface et y adhère par tous ses points en contact. Si l'on chauffe cette plaque, au moyen d'une lampe à l'alcool, l'eau, parvenue à la température de 100 degrés, se convertit en vapeur et passerait tout entière à cet état dans un temps déterminé ; mais si l'on continue d'élever la température de la plaque, au de la de 142° par exemple, le phénomène change de nature ; l'eau cesse d'adhérer à la plaque d'argent, elle ne s'étend plus à sa surface, et semble se replier sur elle-même en prenant la forme d'un sphéroïde aplati de haut en bas. La température jusqu'alors fixée à 100°, s'abaisse subitement à $+ 100^\circ - x$, et reste fixe à ce degré, quel qu'effort que l'on fasse et en plaçant l'appareil dans les conditions calorifiques les plus énergiques. L'évaporation loin d'être augmentée par cette élévation excessive de température, diminue au contraire à ce point qu'une même quantité d'eau pour se réduire en vapeur, exige cinquante fois plus de temps que lorsqu'elle est soumise à la température de l'ébullition. Enfin il s'établit dans le sphéroïde des ondulations régulières, entrecroisées, parfaitement visibles et qui offrent la plus grande analogie avec celles qui sont produites par les corps sonores mis en vibration. La même expérience répétée avec la plupart des autres liquides, donne des résultats qui ne varient que proportionnellement aux limites respectives de leur point normal d'ébullition.

Tel est le phénomène principal : telles sont les nouvelles lois auxquelles obéissent les liquides dès qu'ils cessent d'être soumis à la loi ordinaire de l'équilibre de température, loi qui ne s'exerce que dans des limites déterminées et assez restreintes.

Voici les expériences à l'aide desquelles on peut rendre parfaitement sensibles les moindres détails de ce phénomène. Que l'on prenne un corps très combustible, de l'azotate d'ammoniaque, par exemple, qui s'enflamme à une assez basse température, et qu'on le projette sur une capsule de platine, rougie à l'aide d'un eolipyle ; ce corps entrera en fusion, prendra la forme sphéroïdale, ne brûlera point et ne se décomposera qu'avec beaucoup de lenteur. Retirez alors l'eolipyle, laissez refroidir la plaque jusqu'au degré où cet azotate s'enflamme ordinairement, aussitôt il fusera et s'enflammera : singulier exemple d'un corps très combustible qui ne brûle point dans les circonstances qui sembleraient les plus favorables à sa combustion et qui brûle dès qu'on le soustrait à l'action d'une trop vive chaleur.

Si au lieu d'azotate d'ammoniaque, on projette de l'iode sur une plaque rougie, tant que celle-ci sera très chaude, les vapeurs d'iode seront à peine visibles ; mais si on la laisse refroidir, l'iode s'étalera à la surface en donnant naissance aux belles vapeurs violettes qui le

caractérisent. Enfin, lorsqu'on verse dans une capsule rougie quelques grammes d'eau distillée, le liquide passe rapidement à l'état sphéroïdal; il n'adhère point à la capsule, n'entre pas en ébullition et s'évapore lentement. Si l'on plonge dans le sphéroïde la boule d'un thermomètre, celui-ci accusera invariablement une température plus basse que celle de l'eau bouillante; mais si on laisse refroidir la capsule, le thermomètre se relevera à 100°, l'eau entrera en ébullition tumultueuse et s'évaporerait rapidement dans le tems et les conditions ordinaires.

Ces faits nouveaux et singuliers semblent en opposition manifeste avec les lois connues de la chaleur; d'autres expériences qui sont comme les corollaires des précédentes peuvent servir à montrer toute la portée de ces observations. Ainsi les lois qui régissent les corps passés à l'état sphéroïdal peuvent expliquer certains cas d'explosion des chaudières à vapeur. Si l'on verse deux grammes d'eau distillée dans une petite chaudière sphérique dont le fond est chauffé par un eolipyle et qu'on la bouche fortement; tant que l'eau sera maintenue à l'état sphéroïdal dans son intérieur, il n'y aura pas d'explosion: mais si l'on retire l'eolipyle, un léger bruissement ne tarde pas à annoncer le passage de l'eau à l'état liquide, puis à l'état de vapeur et le bouchon sera lancé violemment dans l'air avec explosion. De même si l'on met de l'eau dans une chaudière d'essai et si on la soumet à une haute température, l'eau ne tardera pas à bouillir et à donner des torrents de vapeur. Si l'alimentation est négligée par une cause quelconque et que la chaudière vienne à rougir, l'eau qu'on y introduira alors acquerra des propriétés nouvelles: elle ne mouillera plus les parois de la chaudière, elle ne s'échauffera pas au delà de 98 degrés et ne donnera que très peu de vapeurs. Mais si l'on diminue l'intensité des feux, ou bien si l'on introduit une grande masse d'eau froide dans la chaudière, cette eau s'étendra sur les parois, se réduira brusquement en vapeurs, dont la tension considérable entraînera infailliblement la rupture et l'explosion de la chaudière.

L'abaissement de la température dans les corps passés à l'état sphéroïdal est une loi générale que l'on peut constater en plongeant la boule d'un thermomètre dans des sphéroïdes d'alcool absolu, d'oxide et de chlorure d'éthyle, d'acide sulfureux et d'un grand nombre d'autres corps. Ce phénomène a donné lieu à un résultat tout-à-fait imprévu et des plus remarquables. On sait que l'acide sulfureux anhydre; liquéfié, entre en ébullition à 11° au-dessous de zéro; l'auteur ayant versé quelques grammes de cet acide dans une capsule de platine rougie au feu, (l'air environnant étant légèrement humide,) l'acide sulfureux prit aussitôt une apparence opaline, perdit sa transparence, se solidifia et l'opérateur vit avec étonnement que ce solide n'était autre chose qu'un morceau de glace. Pour varier l'expérience, il versa dans le même acide à l'état sphéroïdal quelques gouttes d'eau distillée qui se congelèrent rapidement, enfin un très petit matras contenant un gramme d'eau distillée ayant été plongé dans le même

sphéroïde et retiré au bout d'une demi minute contenait également un petit morceau de glace. Pour montrer que les liquides passés à l'état sphéroïdal n'adhèrent plus à la surface du corps échauffant, on fait tomber sur une capsule d'argent ou de cuivre, rougie, de l'acide azotique très concentré; celui-ci roule sur la capsule sans l'attaquer; mais si on laisse refroidir la capsule, il arrive un moment où l'acide attaque le métal avec violence. Autre exemple: un cylindre d'argent chauffé à blanc, étant plongé dans un verre d'eau, on peut voir très distinctement qu'il n'y a pas de contact et que l'eau est maintenue à une certaine distance du cylindre, à mesure que l'équilibre s'établit entre le métal et l'eau, le contact a lieu, un léger sifflement se fait entendre et l'eau entre vivement en ébullition.

Mais ce n'est point à des résultats physiques que se bornent ces curieux phénomènes. Il est évident que ce nouveau mode d'action doit donner lieu à des réactions, à de nouveaux moyens d'analyse et de synthèse chimiques. Certains corps qui ne se décomposent pas à la température de l'ébullition, se décomposent dès qu'ils sont amenés à l'état sphéroïdal; d'autres mis en contact sous l'influence de ce nouvel état moléculaire, se prêtent à de nouvelles combinaisons. Le vin, l'alcool, l'esprit de bois, soumis à l'état sphéroïdal, associent leurs éléments dans un nouvel ordre; l'éther se décompose en dégageant de l'aldehyde; le chlorure d'éthyle décompose l'azotate d'argent; l'ammoniaque dans le même état dissout l'iode; les huiles essentielles, la naphthaline, l'acide benzoïque, l'acide citrique et une foule d'autres substances se transforment et donnent naissance à d'autres produits. Ces exemples suffisent pour montrer tout le parti que les chimistes peuvent tirer de ce nouveau mode d'expérimentation et tout ce qu'il promet dans l'avenir de résultats curieux et inattendus.

Cette forme que prennent les corps soumis à une très haute température et à laquelle se rattachent de nouvelles lois physiques, serait, selon l'auteur de ces recherches, une quatrième modification de la matière, qui ferait suite aux états solide, liquide, gazeux et il lui imposa provisoirement le nom *d'état sphéroïdal*. Il croit en trouver l'origine et la cause dans les vibrations que le calorique communique à la matière. Le passage d'un état à l'autre s'expliquerait par la coïncidence ou la non-coïncidence du mouvement ondulatoire qui a lieu dans le liquide et dans le corps échauffant. L'analogie des vibrations permettrait le contact et l'adhérence, tandis que leur opposition les détruirait. L'adhérence une fois détruite, le liquide se replierait sur lui-même, comme l'eau sur une surface graissée, comme le mercure sur un plan de marbre, ses molécules se rapprocheraient naturellement de la molécule centrale.

Parmi les propriétés des corps amenés à l'état sphéroïdal, il en est deux auxquelles l'auteur attache une importance spéciale. L'une est le pouvoir qu'ils acquièrent de réfléchir le calorique, en se constituant dans un état d'équilibre stable; l'autre est cette faculté de se replier, en vertu d'une attraction particulière vers leur

molécule centrale, comme s'ils étaient réduits à un point matériel isolé dans l'espace, tout en restant soumis à l'attraction terrestre, en sorte que leur forme naturellement sphérique se comprime dans le sens du rayon de la terre. L'auteur tire de ces observations des conséquences fort étendues, applicables à la géologie, à la mécanique terrestre, etc. Il pense que de grandes vérités découleront d'une étude plus approfondie de ces phénomènes. Il pense que la philosophie des sciences fera un pas de géant quand les géomètres analyseront ce phénomène qui a dû se produire à la surface du globe sur une grande échelle à l'époque de son incandescence. Peut-être se produit-il encore à la surface du noyau incandescent de la terre.

Les recherches relatives à l'état sphéroïdal peuvent conduire à l'explication d'un autre ordre de phénomènes. En l'an 241 Sapor, ou Chapour ordonna aux Mages de faire tout ce qui serait en leur pouvoir pour persuader et ramener les dissidents à la foi de leurs ancêtres. Un des Pontifes du culte dominant, Abdurabâd Mabrasphand, offrit de se soumettre dans ce but à ce qu'il appela l'épreuve du feu. Il proposa que l'on répandit sur son corps nud dix-huit livres de cuivre fondu sortant de la fournaise et tout ardent, à condition que s'il n'était point blessé, les incrédules se rendraient à ce prodige. On dit que l'épreuve se fit avec un plein succès et qu'ils furent tous convertis. Les récits merveilleux sur les épreuves par le feu, au moyen âge, et sur les hommes incombustibles dont quelques-uns se montrent encore sur les places publiques se retrouvent partout. Tout le monde a vu ou entendu parler de ces hommes qui courent les pieds nus sur la fonte incandescente, qui plongent la main dans le plomb fondu ou qui appliquent sur leur langue une lame de fer rougie au feu. Ce fait n'aura plus rien d'extraordinaire, si l'on remarque que l'eau, à l'état sphéroïdal, réfléchit le calorique rayonnant et que sa température n'atteint jamais celle de son ébullition. Ainsi, lorsqu'on plonge la main, légèrement humide, dans un métal en fusion, l'humidité qui la recouvre passe à l'état sphéroïdal, réfléchit le calorique rayonnant et ne s'échauffe pas même jusqu'à la température de l'eau bouillante. L'étude du même phénomène donne donc l'explication naturelle de ces faits en apparence si extraordinaires, qui peuvent aujourd'hui se répéter dans les amphithéâtres et les cours publics sans aucun danger pour l'opérateur.

On a vu plus haut que l'eau à l'état sphéroïdal jouait un rôle important dans l'une des causes des explosions fulminantes des chaudières à vapeur. L'auteur poursuivant ses études dans cette direction est parvenu à créer un nouveau système de générateur à vapeur. Le principe général de ce nouveau mode de génération de la vapeur réside dans la division extrême de l'eau au moyen de diaphragmes percés de petits trous et superposés dans l'intérieur de la chaudière. On sait que la matière ne s'évapore que par ses surfaces.

Une chaudière de ce système ayant seulement un demi mètre carré de surface de chauffe évapore de 38 à 40 litres d'eau par heure sous la pression de 10 atmosphères. Dans l'ancien système la même chaudière ne saurait évaporer plus de 10 litres d'eau dans le même temps et sous la même pression. Le rapport est donc :: 1 : 4.

Selon l'auteur, ce nouveau générateur serait absolument inexplosible et destiné à jouer un rôle important dans notre civilisation actuelle en créant une *force domestique*, souvent *gratuite*. En effet, le foyer qui imprimerait le mouvement aux outils de l'atelier donnerait en même temps la chaleur nécessaire, dans tous les climats, à l'existence de l'ouvrier et de sa famille. Ce serait donc à la fois un source de calories et de dynamies avec un seul et même foyer.*

[P.-H. B.]

* On trouvera des détails plus étendus sur ce sujet dans la 3^e édit. d'un opuscule que l'auteur espère pouvoir publier prochainement et qui a pour titre : *Nouvelle Branche de Physique ou Etudes sur les corps à l'Etat sphéroïdal.*

Friday, May 21.

W. R. GROVE, ESQ., M.A., F.R.S., Vice-President,
in the Chair.

B. C. BRODIE, ESQ.

On the Allotropic Changes of Certain Elements.

THE earliest conception of the nature of a chemical substance was limited to the knowledge of the ultimate or elemental particles into which it could be broken up. To this after a time was added that of the proportion in which these elements were combined. But this too proved inadequate to explain the chemical differences of bodies, especially their dynamic differences, that is, the different modes of change of which they are susceptible—why for example, from certain bodies containing many atoms of hydrogen, one of these atoms can readily be removed and replaced by a metal, while no skill has yet effected a similar exchange with a second. The progress of discovery, moreover, established beyond a doubt the existence of a class of bodies consisting of the same elements, combined in the very same proportions, which yet differed in their chemical and physical properties. To meet these and other difficulties, gradually arose an idea new to chemical science, the idea of structure or *chemical form*, in the elaboration of which, chemists of late years have been principally engaged. The way in which this conception has been applied to explain the relation of isomeric bodies may be seen by the following illustration. Representing water as $\begin{smallmatrix} \text{H} \\ \text{H} \end{smallmatrix} \text{O}$, alcohol is represented as $\text{C}_2 \begin{smallmatrix} \text{H}^5 \\ \text{H} \end{smallmatrix} \text{O} = \text{C}_2 \text{H}_6 \text{O}$, and methylic ether as $\text{C}_2 \begin{smallmatrix} \text{H}_3 \\ \text{H}_3 \end{smallmatrix} \text{O} = \text{C}_2 \text{H}_6 \text{O}$. This last substance is identical with alcohol in its elemental constitution, but differs from it in its chemical reactions. This difference is expressed by assuming that the hydro-carbon is differently distributed in the two substances. Looking at the above formulæ, it will be perceived that the conversion of the one substance into the other would be effected by the transference of hydro-carbon from one to the other part of the system. The change in this case has not yet been effected; but in certain other instances we are enabled to effect very analogous transformations, and to recombine the particles in the interior, as it were, of the body itself.

One or two instances were shown of this isomeric metamorphosis, which were so selected as to illustrate the modes by which it could be effected. For example—styrol, (an oil procured by the distillation with water of the liquid storax), by the application of heat, is converted without either the addition or the loss of any chemical substance into a transparent solid, in its ultimate constitution identical with the oil. The formula of the oil is $C_8 H_8$, that of the solid according to Blyth and Hofmann, who first made it a subject of investigation, $C_7 H_7$.

In certain cases these changes may be brought about by a chemical action very analogous to that of fermentation, by which sugar is converted without alteration of weight into carbonic acid and alcohol. There is a body called aldehyde, $C_2 H_4 O$, a very volatile substance boiling almost with the warmth of the hand, and the vapour of which is about $1\frac{1}{2}$ times as heavy as air. By the addition of one drop of sulphuric acid it is converted into a body which boils at a higher temperature than water, and the vapour of which is $4\frac{1}{2}$ times as heavy as air. Our knowledge is too imperfect to state the precise mode in which the elements are re-arranged, but from the density of the vapour we infer that the molecule of the transformed aldehyde contains three times the number of atoms of the original body and is $C_6 H_{12} O_3$. Oil of turpentine is in a most remarkable degree susceptible of these metamorphoses. By the action of sulphuric acid it is converted into oils, isomeric with it, but each differing from it in some one or more properties. Great heat is evolved during the change, due doubtless to the chemical combination which is taking place. In certain cases, as, for example, the formation of paracyanogen by the decomposition of cyanide of silver, this heat is so great as to cause the vivid ignition of the substance.

Acquaintance with these facts is necessary to view in their scientific connexion certain phænomena of the elemental bodies which go under the name of *allotropy*, and which are to them, precisely what isomeric phænomena are to compound substances. This allotropy has been observed in the case of many elements, carbon, boron, silicon, selenium, sulphur, phosphorus, arsenic, and possibly oxygen—from these sulphur and phosphorus were selected as presenting points of peculiar interest.

At a few degrees above the boiling point of water, Sulphur melts to a transparent yellow fluid; at about $160^\circ C$. it changes in appearance, becoming red, and between 220° and $250^\circ C$. it becomes deep red and viscid. Heated beyond this point it again becomes liquid, and just before it boils appears as a deep black fluid. These changes in the sulphur have been connected with certain thermic phænomena, which leave no doubt but that they are truly transitions from one allotropic form to another. During cooling, sulphur passes through the same changes, but in an inverse order; and it has been observed that the sulphur does not cool by regular

gradation, but that at certain points its temperature is stationary, or falls much slower than at other points; this can only be explained by assuming a developement of heat from the sulphur itself, which compensates for that which it loses; and this developement of heat takes place just about the points of transition from one state to another. By cooling suddenly the viscid sulphur, it may be retained for a long time in the form of a transparent elastic substance, which ultimately solidifies to a sulphur differing in many respects from the ordinary modification of the body, especially in being in great measure insoluble in bisulphide of carbon.—An experiment was shewn of the reconversion of this peculiar sulphur into the viscid form; this can be effected without melting the body by a proper regulation of the temperature.

It had been long known that Phosphorus exposed to sun-light assumed a red colour. Berzelius suspected this to be an allotropic modification of the element; and the experiments of Schrötter, who produced the same body by the action of heat, have established this view and enable us to procure this phosphorus in large quantities. In its chemical properties, as well as in appearance, this red phosphorus is entirely different from the ordinary modification.

The change thus produced by heat can also be effected by chemical agency. By the action of iodine ordinary phosphorus can be converted in large quantities into the allotropic modification. This can be done by projecting iodine into phosphorus melted under strong hydrochloric acid, or into phosphorus simply melted in a tube, and subsequently heating the substance. The iodine is employed in very small quantities. It first dissolves in the liquid phosphorus; at a certain point as the temperature is raised a violent evolution of heat takes place, which causes a kind of explosion in the substance, and the mass of the phosphorus passes at this moment into the other condition. A small quantity of iodine will in this manner convert (if sufficient time be allowed) an absolutely unlimited quantity of phosphorus.*

On a former occasion certain experiments were shown as evidence that the formation of oxygen, and indeed of other elements, was a chemical combination of particles of the same nature as the formation of a compound substance, and that the two classes of bodies had a similar molecular constitution.† By the phenomena of allotropy, other analogies are brought out between them, which lead to a similar belief. The similarity is so great between the facts in the

* The theory of this action and the detailed experiments are given in a Paper on the action of Iodine upon Phosphorus, read by the Author before the Chemical Society on the 21st of June.

† See on this subject the Author's Paper in the Philosophical Transactions, 1850, "on the Condition of certain Elements at the moment of Chemical Change."

two cases, — they are produced by the same means, by the alteration of temperature and by chemical action, — they are attended with the same evolution of heat, — that it is reasonable to refer them to the same cause. In the case of the compound substance we have the most direct evidence that the allotropic conversion is the re-combination of the particles of the substance and the transition from one chemical type to another. The inference is that the change in the case of the elements is of the same nature, and that phosphorus and sulphur are molecular groups capable of this re-arrangement and re-distribution.

[B. C. B.]

Friday, June 11.

SIR CHARLES FELLOWS, Vice-President, in the Chair.

PROFESSOR FARADAY,

On the Physical Lines of Magnetic Force.

ON a former occasion, (Jan. 23, 1852, see p. 105) certain lines about a bar magnet were described and defined (being those which are depicted to the eye by the use of iron filings sprinkled in the neighbourhood of the magnet), and were recommended as expressing accurately the nature, condition, direction, and amount of the force in any given region either within or outside of the bar. At that time the lines were considered in the abstract. Without departing from or unsettling any thing then said, the enquiry is now entered upon of the possible and probable *physical existence* of such lines. Those who wish to reconsider the different points belonging to these parts of magnetic science may refer to two papers in the first part of the Phil. Trans. for 1852 for data concerning the *representative* lines of force, and to a paper in the Phil. Mag. 4th Series, 1852, vol. iii. p. 401, for the argument respecting the *physical* lines of force.

Many powers act manifestly at a distance; their physical nature is incomprehensible to us; still we may learn much that is real and positive about them, and amongst other things something of the condition of the space between the body acting and that acted upon, or between the two mutually acting bodies. Such forces are presented to us by the phenomena of gravity, light, electricity, magnetism, &c. These when examined will be found to present remarkable differences in relation to their respective lines of forces; and at the same time that they establish the existence of real physical lines in some cases, will facilitate the consideration of the question as applied especially to magnetism.

When two bodies, *a*, *b*, gravitate towards each other, the line in which they act is a straight line, for such is the line which either would follow if free to move. The attractive force is not altered, either in *direction* or *amount*, if a third body is made to act by gravitation or otherwise upon either or both of the two first. A balanced cylinder of brass gravitates to the earth with a weight exactly the same, whether it is left like a pendulum freely to hang towards it, or whether it is drawn aside by other attractions or by tension, whatever the amount of the latter may be. A new gravitating force may be exerted upon *a*, but that does not in the least affect the amount of power which it exerts towards *b*. We have no evidence that *time*

enters in any way into the exercise of this power, whatever the distance between the acting bodies, as that from the sun to the earth, or from star to star. We can hardly conceive of this force in one particle by itself; it is when two or more are present that we comprehend it: yet in gaining this idea we perceive no difference in the character of the power in the different particles; all of the same kind are *equal, mutual, and alike*. In the case of gravitation, no effect which sustains the idea of an independent or physical line of force is presented to us; and as far as we at present know, the line of gravitation is merely an ideal line representing the direction in which the power is exerted.

Take the Sun in relation to another force which it exerts upon the earth, namely, its illuminating or warming power. In this case rays (which are lines of force) pass across the intermediate space; but then we may affect these lines by different media applied to them in their course. We may alter their direction either by reflection or refraction; we may make them pursue curved or angular courses. We may cut them off at their origin and then search for and find them before they have attained their object. They have a relation to *time*, and occupy 8 minutes in coming from the sun to the earth: so that they may exist independently either of their source or their final home, and have in fact a clear distinct physical existence. They are in extreme contrast with the lines of gravitating power in this respect; as they are also in respect of their condition at their terminations. The two bodies terminating a line of gravitating force are alike in their actions in every respect, and so the line joining them has like relations in both directions. The two bodies at the terminals of a ray are utterly unlike in action; one is a source, the other a destroyer of the line; and the line itself has the relation of a stream flowing in one direction. In these two cases of gravity and radiation, the difference between an abstract and a physical line of force is immediately manifest.

Turning to the case of Static Electricity we find here attractions (and other actions) at a distance as in the former cases; but when we come to compare the attraction with that of gravity, very striking distinctions are presented which immediately affect the question of a physical line of force. In the first place, when we examine the bodies bounding or terminating the lines of attraction, we find them as before, mutually and equally concerned in the action; but they are not alike: on the contrary, though each is endued with a force which speaking generally is of the like nature, still they are in such contrast that their actions on a third body in a state like either of them are precisely the reverse of each other,— what the one attracts the other repels; and the force makes itself evident as one of those manifestations of power endued with a dual and antithetical condition. Now with all such dual powers, attraction cannot occur unless the two conditions of force are present and in face of each other through the lines of force. Another essential limitation is that

these two conditions must be exactly equal in amount, not merely to produce the effects of attraction, but in every other case ; for it is impossible so to arrange things that there shall be present or be evolved more electric power of the one kind than of the other. Another limitation is that they must be in physical relation to each other ; and that when a positive and a negative electrified surface are thus associated, we cannot cut off this relation except by transferring the forces of these surfaces to equal amounts of the contrary forces provided elsewhere. Another limitation is that the power is definite in amount. If a ball *a* be charged with 10 of positive electricity it may be made to act with that amount of power on another ball *b* charged with 10 of negative electricity ; but if 5 of its power be taken up by a third ball *c* charged with negative electricity, then it can only act with 5 of power on ball *a*, and that ball must find or evolve 5 of positive power elsewhere : this is quite unlike what occurs with gravity, a power that presents us with nothing dual in its character. Finally the electric force acts in curved lines. If a ball be electrified positively and insulated in the air, and a round metallic plate be placed about 12 or 15 inches off, facing it and uninsulated, the latter will be found, by the necessity mentioned above, in a negative condition ; but it is not negative only on the side facing the ball but on the other or outer face also, as may be shewn by a carrier applied there, or by a strip of gold or silver leaf hung against that outer face. Now the power affecting this face does not pass through the uninsulated plate, for the thinnest gold leaf is able to stop the inductive action, but round the edges of the face and therefore acts in curved lines. All these points indicate the existence of physical lines of electric force : — the absolutely essential relation of positive and negative surfaces to each other, and their dependence on each other contrasted with the known mobility of the forces, admit of no other conclusion. The action also in curved lines must depend upon a physical line of force. And there is a third important character of the force leading to the same result, namely, its affection by media having different specific inductive capacities.

When we pass to Dynamic Electricity the evidence of physical lines of force is far more patent. A voltaic battery, having its extremities connected by a conducting medium, has what has been expressively called a current of force running round the circuit, but this current is an axis of power having equal and contrary forces in opposite directions. It consists of lines of force which are compressed or expanded according to the transverse action of the conductor, which changes in direction with the form of the conductor, which are found in every part of the conductor, and can be taken out from any place by channels properly appointed for the purpose ; and nobody doubts that they are physical lines of force.

Finally as regards a Magnet, which is the object of the present discourse. A magnet presents a system of forces perfect in itself,

and able, therefore, to exist by its own mutual relations. It has the dual and antithetic character belonging to both static and dynamic electricity; and this is made manifest by what are called its polarities, *i. e.* by the opposite powers of like kind found at and towards its extremities. These powers are found to be absolutely equal to each other; one cannot be changed in any degree as to amount without an equal change of the other; and this is true when the opposite polarities of a magnet are not related to each other, but to the polarities of other magnets. The polarities, or the *northness* and *southness*, of a magnet are not only related to each other, through or within the magnet itself, but they are also related externally to opposite polarities, (in the manner of static electric induction) or they cannot exist; and this external relation involves and necessitates an exactly equal amount of the new opposite polarities to which those of the magnet are related. So that if the force of a magnet *a* is related to that of another magnet *b*, it cannot act on a third magnet *c* without being taken off from *b*, to an amount proportional to its action on *c*. The lines of magnetic force are shewn by the moving wire to exist both within and outside of the magnet; also they are shewn to be closed curves passing in one part of their course through the magnet; and the amount of those within the magnet at its equator is exactly equal in force to the amount in any section including the whole of those on the outside. The lines of force outside a magnet can be affected in their direction by the use of various media placed in their course. A magnet can in no way be procured having only one magnetism, or even the smallest excess of northness or southness one over the other. When the polarities of a magnet are not related externally to the forces of other magnets, then they are related to each other: *i. e.* the northness and southness of an isolated magnet are externally dependent on and sustained by each other.

Now all these facts, and many more, point to the existence of physical lines of force external to the magnets as well as within. They exist in curved as well as in straight lines; for if we conceive of an isolated straight bar magnet, or more especially of a round disc of steel magnetised regularly, so that its magnetic axis shall be in one diameter, it is evident that the polarities must be related to each other externally by curved lines of force; for no straight line can at the same time touch two points having northness and southness. Curved lines of force can, as I think, only consist with physical lines of force.

The phenomena exhibited by the moving wire confirm the same conclusion. As the wire moves across the lines of force, a current of electricity passes or tends to pass through it, there being no such current before the wire is moved. The wire when quiescent has no such current, and when it moves it need not pass into places where the magnetic force is greater or less. It may travel in such a course that if a magnetic needle were carried through the same course it would

be entirely unaffected magnetically, *i. e.* it would be a matter of absolute indifference to the needle whether it were moving or still. Matters may be so arranged that the wire when still shall have the same diamagnetic force as the medium surrounding the magnet, and so in no way cause disturbance of the lines of force passing through both; and yet when the wire moves, a current of electricity shall be generated in it. The mere fact of motion cannot have produced this current: there must have been a state or condition around the magnet and sustained by it, within the range of which the wire was placed; and this state shews the physical constitution of the lines of magnetic force.

What this state is or upon what it depends cannot as yet be declared. It may depend upon the ether, as a ray of light does, and an association has already been shewn between light and magnetism. It may depend upon a state of tension, or a state of vibration, or perhaps some other state analogous to the electric current, to which the magnetic forces are so intimately related. Whether it of necessity requires matter for its sustentation will depend upon what is understood by the term matter. If that is to be confined to ponderable or gravitating substances, then matter is not essential to the physical lines of magnetic force any more than to a ray of light or heat; but if in the assumption of an ether we admit it to be a species of matter, then the lines of force may depend upon some function of it. Experimentally mere space is magnetic; but then the idea of such mere space must include that of the ether, when one is talking on that belief; or if hereafter any other conception of the state or condition of space rise up, it must be admitted into the view of that, which just now in relation to experiment is called mere space. On the other hand it is, I think, an ascertained fact that ponderable matter is not essential to the existence of physical lines of magnetic force.

[M. F.]

Friday, January 21.

WILLIAM POLE, ESQ., M.A., F.R.S., Treasurer and Vice-President,
in the Chair.

PROFESSOR FARADAY,

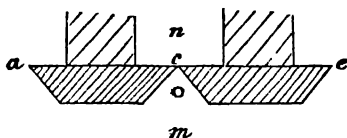
Observations on the Magnetic Force.

INASMUCH as the general considerations to be brought forward had respect to those great forces of the globe, exerted by it, both as a mass and through its particles, namely, Magnetism and Gravitation, the attention was first recalled briefly to certain relations and differences of the two which had been insisted upon on former occasions. Both can act at a distance, and doubtless at any distance : but whilst gravitation may be considered as simple and unpolar in its relations, magnetism is dual and polar. Hence *one* gravitating particle or system cannot be conceived to act by gravitation, as a particle or system, on itself ; whereas a magnetic particle or system, because of the dual nature of its force, can have such a self relation. Again, either polarity of the magnetic force can act either by attraction or repulsion ; and not merely so, but the joint or *dual* action of a magnet can act also either by attraction or repulsion, as in the case of paramagnetic and diamagnetic bodies : the action of gravity is always that of attraction. As a further consequence of the difference in character of the powers, little or no doubt was entertained regarding the existence of physical lines of force* in the cases of dual powers, as electricity and magnetism ; but in respect of gravitation the conclusion did not seem so sure. As some further relations of the sun and the earth would have finally to be submitted, the audience were reminded, by the use of Arago's idea, of the relative magnitude of the two ; for, supposing that the centres of the two globes were made to coincide, the sun's body would not only extend as far as the moon, but nearly as far again, its bulk being about seven times that of a globe which should be girdled by the moon's orbit.

For the more careful study of the magnetic power a torsion balance had been constructed, which was now shown and its mode of operation explained. The torsion wire was of hard drawn platinum, 24 inches in length, and of such diameter that 28.5 inches

* Proceedings of the Royal Institution, June 11, 1852, p. 216, also *Philosoph. Mag.* 4th Series, 1852, III. p. 401.

weighed one grain. It was attached as usual to a torsion head and index. The horizontal beam was a small glass tube terminated at the object end by a glass hook. The objects to be submitted to the magnetic force were either cylinders of glass with a filament drawn out from each, so as to make a long stiff hook for suspension from the beam; or cylindrical bulbs of glass, of like shape, but larger size, formed out of glass tube; or other matters. The fine tubular extremities of the bulbs being opened the way through was free from end to end; the bulbs could then be filled with any fluid or gas, and be re-submitted many times in succession to the magnetic force. The source of power employed was at first a large electro-magnet; but afterwards, in order to be certain of a constant power, and for the advantage of allowing any length of time for the observations, the great magnet, constructed by M. Logeman upon the principles developed by Dr. Elias, (and which, weighing above 100 lbs. could support 430 lbs. according to the report of the Great Exhibition Jury), was purchased by the Royal Institution and used in the enquiries. The magnet was so arranged that the axis of power was five inches below the level of the glass beam, the interval being traversed by the suspension filament or hook, spoken of above. The form and position of the terminations of soft iron are shewn in plan by the diagram upon a scale of $\frac{1}{16}$, and also the place of the object. All



this part is enclosed in the box which belongs to and carries the torsion balance, which box is governed by six screws fixed upon the magnet table; and as both the box and the table have lines and scales marked upon them, it is easy to adjust the former on the latter so that the beam shall be over and parallel to the line *a, e* with the point of suspension over *c*; or, by moving the whole box parallel to itself towards *m*, to give the point of suspension any other distance from the angle *c*. As already said, the objects were constructed with a suspension filament of such length as to make them coincide in height with the angle in the magnetic field. When suspended on the beam they were counterpoised by a ring or rings of lead on the further arm of the beam. These when required were moved along the beam until the latter was horizontal; and that state was ascertained by a double arm support, which sustained the beam when out of use, brought it into a steady state when moving, and delivered it into a condition of freedom when required. The motion of the box to the right or left, so as to place the object in the middle of the magnetic angle, was given by two

of the screws before spoken of; the motion to the given distance from *c*, by the other four.

Supposing the distance from *c* towards *m* to be adjusted to 0.6 of an inch, when the beam was loaded above, and no object before the magnet (the beam having been of course previously adjusted to its normal position and the torsion index placed at zero), it then remained to determine the return of the beam to its place when the object had been suspended on it and repelled; this was done in the following manner. A small plane reflector is fixed on the beam, near its middle part, under the point of suspension: a small telescope associated with a divided scale is placed about 6 feet from the reflector, and in such a position that when the beam is in its right place, a given degree in the scale coincides with the fine wire in the telescope. Of course the scale appears to pass by the wire as the beam itself moves, and with a double angular velocity, because of the reflexion. As it is easy to read to the fiftieth and even to the hundredth of an inch in this way, and as each degree occupies apparently 2.4 inches with the radius of 6 feet, so an angular motion, or difference of $\frac{1}{120}$ of a degree could be observed; and as the radius of the arm of the beam carrying the object was 6 inches, such a quantity there would be less than $\frac{1}{2000}$ of an inch: i. e. the return of the beam to its first or normal position by the torsion force put on to counteract the repulsion, could be ascertained to within that amount. When an object was put on the adjusted beam, if diamagnetic it was repelled; and then, as the observer sat at the telescope, he, by means of a long handle, a wheel, and pinion, put on torsion until the place of the beam was restored; and afterwards the amount of torsion read off on the graduated scale became the measure in degrees of the repulsive force exerted. At the time of real observations, the magnet, balance, and telescope were all fixed in a basement room, upon a stone floor. But it is unnecessary to describe here the numerous precautions required in relation to the time of an observation, the set of the suspension wire by a high torsion, the possible electricity of the object or beam by touch, the effect of feeble currents of air within the box, the shape of the object, the precaution against capillary action when fluids were employed as media, and other circumstances; or the use of certain stops, and the mode of procedure in the cases of paramagnetic action; — the object being at present to present only an intelligible view of the principles of action.

When a body is submitted to the power of a magnet, it is affected, as to the result, not merely by the magnet, but also by the medium surrounding it; and even if that medium be changed for a vacuum, the vacuum and the body still are in like relation to each other. In fact the result is always differential; any change in the medium changes the action on the object, and there are abundance of substances which when surrounded by air are repelled, and when by water, are attracted, upon the approach of a magnet. When a certain small

glass cylinder weighing only 66 grains surrounded by air was submitted on the torsion balance to the Logeman magnet at the distance of 0.5 of an inch from the axial line, it required 15° of torsion to overcome the repulsive force and restore the object to its place. When a vessel of water was put into the magnetic field, and the experiment repeated, the cylinder being now in the water was attracted, and $54^{\circ}.5$ of torsion were required to overcome this attraction at the given distance of 0.5. If the vessel had contained a fluid exactly equal in diamagnetic power to the cylinder of glass, neither attraction nor repulsion would have been exerted on the latter, and therefore the torsion would have been 0° . Hence the three bodies, air, glass (the especial specimen), and water, have their relative force measured in relation to each other by the three experimental numbers 15° , 0° , and $54^{\circ}.5$. If other fluids are taken, as oil, ether, &c. and employed as the media surrounding the *same glass cylinder*, then the degrees of torsion obtained with each of them respectively, shews its place in the magnetic series. It is the principle of the hydrometer or of Archimedes in respect of gravity applied in the case of the magnetic forces. If a different cylinder be employed of another size or substance, or at a different distance, the torsion numbers will be different, and the zero (given by the cylinder) also different; but the media (with an exception to be made hereafter) will have the same relation to each other as in the former case. Therefore to bring all the experimental results into one common relation, a centigrade scale has been adopted bounded by air and water at common temperatures or 60° F. For this purpose every separate series of results made under exactly the same circumstances included air and water; and then all the results of one series were multiplied by such a number as would convert the difference between air and water into 100° : in this way the three results given above, become $21^{\circ}.6$; 0° ; and $78^{\circ}.4$. By such a process the magnetic intervals between the bodies are obtained on the centigrade scale, but the true zero is not as yet determined. Either water, or air, or the glass, may be assumed as the zero, the intervals not being in any way dependent upon that point, but the results will then vary in expression thus:—

Air	.	0°	.	$21^{\circ}.6$.	100°
Glass	.	$21^{\circ}.6$.	0°	.	$78^{\circ}.4$
Water	.	100°	.	$78^{\circ}.4$.	0°

all above the zero being paramagnetic, and all below diamagnetic, in relation to it. I have adopted a vacuum as the zero in the table of results to be given hereafter.

In this manner it is evident that upon principle, any solid, whatever its size, shape, or quality, may be included in the list, by its subjection to a magnet in air and in water or in fluids already related to these: also that any fluids may be included by the use of the *same* immersed solid body for them, air and water; and also that by using the same vessel, as for instance the same glass bulb,

and filling it successively with various gases and fluids, including always air and water in each series, these included bodies may then have their results reduced and be entered upon the list. The following is a table of some substances estimated on the centigrade scale, and though there are many points both of theory and practice yet to be wrought out, as regards the use of the torsion balance described, so that the results can only be recorded as approximations, yet even now the average of three or four careful experiments, gives an expression for any particular substance under the same conditions of distance, power, &c. near upon and often within a degree of the place assigned to it. The powers are expressed for a distance of 0.6 of an inch from the magnetic axis of the magnet as arranged and described, and, of course, for *equal volumes* of the bodies mentioned. The extreme decimal places must not be taken as indicating accuracy, except as regards the record of the experiments: they are the results of calculation. Hydrogen, nitrogen, and perhaps some other of the bodies near zero, may ultimately turn out to be as a vacuum; it is evident that a very little oxygen would produce a difference, such as that which appears in nitrogen gas. The first solution of copper mentioned was colourless, and the second the same solution oxidized by simple agitation in a bottle with air, the copper, ammonia, and water being in both the same.

Prot-ammo. of copper	134.23	Camphor . . .	82.59
Per-ammo. of copper	119.83	Camphine . . .	82.96
Oxygen . . .	17.5	Linseed oil . . .	85.56
Air . . .	3.4	Olive oil . . .	85.6
Olefiant gas . . .	0.6	Wax . . .	86.73
Nitrogen . . .	0.3	Nitric acid . . .	87.96
Vacuum . . .	0.0	Water . . .	96.6
Carbonic acid gas . . .	0.0	Solution of Ammonia .	98.5
Hydrogen . . .	0.1	Bisulphide of carbon .	99.64
Ammonia gas . . .	0.5	Sat. sol. Nitre . . .	100.08
Cyanogen . . .	0.9	Sulphuric acid . . .	104.47
A Glass . . .	18.2	Sulphur . . .	118.
Pure Zinc . . .	74.6	Chloride of Arsenic . .	121.73
Ether . . .	75.3	Fused Borate lead . .	136.6
Alcohol absolute . . .	78.7	Phosphorus . . .	
Oil of Lemons . . .	80.	Bismuth . . .	1967.6

Plücker in his very valuable paper* has dealt with bodies which are amongst the highly paramagnetic substances, and his estimate of power is made for equal weights.

One great object in the construction of an instrument delicate as that described, was the investigation of certain points in the philosophy of magnetism; and amongst them especially that of the right application of the law of the inverse square of the distance as the universal law of magnetic action. Ordinary magnetic

* Taylor's Scientific Memoirs, V. 713, 730.

action may be divided into two kinds, that between magnets permanently magnetized and unchangeable in their condition, and that between bodies of which one is a permanent unchangeable magnet, and the other, having no magnetic state of its own, receives and retains its state only whilst in subjection to the first. The former kind of action appears in the most rigid and pure cases, to be subject to that law; but it would be premature to assume beforehand, and without abundant sufficient evidence, that the same law applies in the second set of cases also; for a hasty assumption might be in opposition to the truth of nature, and therefore injurious to the progress of science, by the creation of a preconceived conclusion. We know not whether such bodies as oxygen, copper, water, bismuth, &c., owe their respective paramagnetic and diamagnetic relation to a greater or less facility of conduction in regard to the lines of magnetic force, or to something like a polarity of their particles or masses, or to some as yet unsuspected state; and there is little hope of our developing the true condition, and therefore the cause of magnetic action, if we assume beforehand the unproved law of action and reject the experiments that already bear upon it:—for Plücker has distinctly stated as the fact, that diamagnetic force increases more rapidly than magnetic force, when the power of the dominant magnet is increased; and such a fact is contrary to the law above enunciated. The following are further results in relation to this point.

When a body is submitted to the great unchanging Logeman magnet in air and in water, and the results are reduced to the centigrade scale, the relation of the three substances remain the same for the same distance, but not for *different* distances. Thus when a given cylinder of flint glass was submitted to the magnet surrounded by air and by water, at the distance of 0.3 of an inch as already described, it proved to be diamagnetic in relation to both; and when the results were corrected to the centigrade scale, and water made zero, it was $9^{\circ}.1$ below, or on the diamagnetic side of water. At the distance 0.4 of an inch it was $10^{\circ}.6$ below water: at the distance of 0.7 it was $12^{\circ}.1$ below water. When a more diamagnetic body, as heavy glass, was employed, the same result in a higher degree was obtained; for at the distance of 0.3 it was $37^{\circ}.8$ below water, and at that of 0.8 it was $48^{\circ}.6$ beneath it. Bismuth presented a still more striking case, though, as the volume of the substance was necessarily small, equal confidence cannot be placed in the exactitude of the numbers. The results are given below for the three substances, air being always 100° and water 0° ; the first column of figures for each substance contain the distance *

* A given change of distance necessarily implies change in degree of force, and change in the forms of the lines of force; but it does not imply always the same amount of change. The forces are not the same at the same distance of 0.4 of an inch in opposite directions from the axial line towards *m* and *n* in the figure, page 230, nor at any other equal moderate distance; and though by increase and diminution of distance the change is in the same direction, it

in tenths of an inch from the axial line of the magnetic field, and the second, the place in centigrade magnetic degrees below water.

Flint Glass.	Heavy Glass.	Bismuth.
0.3 — 9°1	0.3 — 37°8	0.6 — 1871°
0.4 — 10.6	0.4 — 38.6	1.0 — 2734.
0.5 — 11.1	0.6 — 40.0	1.5 — 3626.
0.6 — 11.2	0.8 — 48.6	
0.7 — 12.1	1.0 — 51.5	
	1.2 — 65.6	

The result here is that the greater the distance of the diamagnetic bodies from the magnet, the more diamagnetic it is in relation to water, taking the interval between water and air as the standard: and it would further appear, if an opinion may be formed from so few experiments, that the more diamagnetic the body compared to air and water, the greater does this difference become. At first it was thought possible that the results might be due to some previous state induced upon the body, by its having been nearer to or further from the magnet: but it was found that whether the progress of the experiments was from small to large distances, or the reverse; or whether, at any given distance, the object was previous to the measurement held close up to the magnet or brought from a distance, the results were the same;—no evidence of a temporary induced state could in any of these ways be found.

It does not follow from the experiments, if they should be sustained by future researches, that it is the glass or the bismuth only that changes in relation to the other two bodies. It may be the oxygen of the air that alters, or the water, or more probably all these bodies: for if the result be a true and natural result in these cases, it is probably common to all substances. The great point is that the three bodies concerned, air, water, and the subject of the experiment, alter in the degree of their magnetic relations to *each other*; at different given distances from the magnet the ratio of their magnetic power does not, according to the experiments, remain the same; and if that result be confirmed, then it cannot be included by a law of action which is inversely as the square of the distance. A hydrometer floating in a fluid and subject to the gravity of the earth alone, would (other things being the same) stand at the same point, whether at the surface of the earth, or removed many diameters of the earth from it, because the action of gravity is inversely as the square of the distance: but if we suppose the substance of the hydrometer and the fluid to differ magnetically, as water and bismuth does, and the earth to act as a magnet instead of by gravity, then the hydrometer would, according to the experiments, stand at a different point for different distances, and if so could not be subject to the former law.

is not in the same proportion. By fitly arranged terminations, it may be made to alter with extreme rapidity in one direction, and with extreme slowness or not at all in another.

The cause of this variation in the ratio of the substances one to another, if it be finally proved, has still to be searched out. It may depend in some manner upon the *forms* of the lines of magnetic force, which are different at different distances; or not upon the forms of the lines but the *amount* of power at the different distances; or not upon the mere amount, but on the circumstance that in every case the body submitted to experiment has lines of *different degrees of force* passing through different parts of it, (for however different the magnetic or diamagnetic conditions of a body and the fluid surrounding it, they would not move at all in relation to each other in a field of equal force): but whatever be the cause, it will be a concomitant of magnetic actions; and therefore ought to be included in the results of any law by which it is supposed that these actions are governed.

It has not yet been noticed that these general results appear to be in direct opposition to those of Plücker, who finds that diamagnetic power increases more rapidly than magnetic power with increase of force. But such a circumstance, if both conclusions be accordant with facts, only shews that we have yet a great deal to learn about the physical nature of magnetic force; and we must not shut our eyes to the first feeble glimpses of these effects, because they are inconsistent on both sides with our assumed laws of action; but rather seize them, as hoping that they will give us the key to the truth of nature. Bodies, when subject to the power of the magnet appear to acquire a new physical state, which varies with the distance or the power of the magnet. Each body may have its own rate of increase and decrease; and that may be such as to connect the extreme effect of Plücker, amongst paramagnetic bodies on the one hand, and the extreme effects amongst diamagnetic bodies now described, on the other; and when we understand all this rightly, we may see the apparent contradiction become harmony, though it may not conform to the law of the inverse square of the distance as we now try to apply it.

Plücker has already said, because of his observations regarding paramagnetic and diamagnetic force, that no correct list of magnetic substances can be given. The same consequence follows, though in a different direction from what has now been stated, and hence the reservation before made (p. 232). Still the former table is given as an approximation, and it may be useful for a time. Before leaving his first account of recent experimental researches, it may be as well to state that they are felt to be imperfect and may perhaps even be overturned; but, that as such a result is not greatly anticipated, it was thought well to present them to the Members of the Royal Institution and the scientific world, if peradventure they might excite criticism and experimental examination, and so aid in advancing the cause of physical science.

On a former occasion * the existence of *physical* lines of force in

* Proceedings of the Royal Institution, p. 216.

relation to magnetism and electricity was inferred from the dual nature of these powers, and the necessity in all cases and at all times of a relation and dependence between the polarities of the magnet, or the positive and negative electrical surfaces. With respect to gravity a more hesitating opinion was expressed, because of the difficulty of observing facts having any relation to *time*, and because two gravitating particles or masses did not seem to have any necessary dependence on each other for the existence or excitement of their mutual power*. On the present occasion a passage was quoted from Newton which had since been discovered in his works, and which, shewing that he was an unhesitating believer in physical lines of gravitating force, must from its nature, rank him amongst those who sustain the physical nature of the lines of magnetic and electrical force: it is as follows, in words written to Bentley: † "That gravity should be innate, inherent and essential to matter, so that one body may act upon another at a distance through a *vacuum*, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity, that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my-readers."

Finally, reference was made to Sabine's remarkable observation, sustained as it has been by Wolf, Gautier, and others, of certain coincidences existing between the appearance of solar spots and the diurnal variation of the magnetism of the earth. SCHWABE has been engaged in carefully observing the spots on the Sun since the year 1826. He has found them gradually to increase in number and size from year to year, and then decrease; then again increase, again to decrease, and so on in a regular period of about ten years. The following is a part of his table ‡ giving the years of the maxima and minima of spots:

	Groups in the year.	Days of no spots.	Days of observation.
1826			
1828	.. 225 0 282
1833	. . . 33 139 267
1837	.. 333 0 168
1843	. . . 34 149 312
1848	.. 330 0 278
1851			

* Philosophical Magazine, 4th Series, 1852, III. 403. (3246.)

† Newton's Works, Horsley's edition, 4to. 1783, Vol. IV. p. 438, or the Third Letter to Bentley.

‡ Humboldt's Cosmos, III. 291, 292. Bibliothèque Universelle, 1852, XX. 184.

LAMONT (Dec. 1851)* was induced by recent researches in atmospheric magnetism, to examine the daily magnetic variation in declination, and found that, as a whole, it increased and diminished, and then increased again, having a regular variation of about ten years: the year 1844 was given as having a minimum variation of 6'.61 and the year 1848 as presenting a maximum variation of 11'.15.

SABINE† (March, 1852) in searching for periodical laws amongst the mean effects of the larger magnetic disturbances, found a 'simultaneous period of increase and decrease both at Hobarton and Toronto, on opposite sides of the globe; the minimum effect was in 1843, and the maximum effect in 1848, according therefore almost exactly with Lamont's observations at Munich. But, besides that, he pointed out the extraordinary circumstance that this similar variation of the daily magnetic declination is the same in length of period as that discovered by M. Schwabe for the solar spots; and still more, that the maxima and minima of these two most different phenomena coincide; for 1843 presents the least diurnal variation and the smallest number of solar spots, and 1848 the largest magnetic variation and the greatest number of solar obscurations. He has also observed that the same period of increase and decrease exists with the same epochs in the diurnal variation of the magnetic inclination of the earth's magnetic force in both hemispheres. The phenomenon is general both as regards all the magnetic elements, and in parts of the globe most distant from each other.

GAUTIER appears to have been struck with the same coincidence; but did not publish his idea until July 1852.‡ WOLF of Berne, who has sought far into the history of the sun spots, had the same thought, publishing it first at the end of July or beginning of August, 1852.§ He endeavours to trace the general condition of the spots from the year 1600, and concludes that the true length of the period is 11.11 years. As it is impossible to conceive such a coincidence in the length of the period and the time of the maxima and minima of these two greatly differing phenomena, without believing in some relation of them to a common cause; so, the observation of such a coincidence at this moment ought to urge us more than ever into an earnest and vigorous investigation of the true and intimate nature of magnetism; by means of which we now have hopes of touching in a new direction, not merely this remarkable force of the earth, but even the like powers of the sun itself.

[M. F.]

* Poggendorff, *Annalen*, LXXXIV. p. 572.

† *Philosophical Transactions*, 1852, page 103.

‡ *Bibliothèque Universelle*, 1852, XX. 189.

§ *Proceedings of Natural Society of Berne*, Nos. 245—247.

Friday, Jan. 28.

W. R. GROVE, Esq., M.A., F.R.S., Vice-President,
in the Chair.

PROFESSOR WILLIAMSON,

On Gerhardt's discovery of Anhydrous Organic Acids.

THE discovery by M. Gerhardt of a number of anhydrous organic acids has thrown so much light on one of the most important questions of chemical philosophy, that it constitutes one of the most remarkable illustrations of the manner in which the rich materials of organic chemistry may be brought to bear on the explanation of the phenomena of chemical action and the laws of chemical combination.

It is not unworthy of remark, that the bodies prepared by Gerhardt had for some years past been supposed to exist ready formed in combination with water and other bases, and that the chief objection to that supposition was founded on the circumstance of their never having been separated from such combination, and presented in an isolated form. In fact Gerhardt has supplied the very link in the chain, which was expected to constitute evidence for a familiar theory of the constitution of salts. But the process by which the result was attained is even more important than the result itself, and has led to our drawing from that result a conclusion different from that which was generally expected. Chemistry aims at discovering the nature of that action by which substances of opposite properties undergo those remarkable changes which we call chemical combination; and it naturally follows from this view of its objects, that chemical science is more advanced by the discovery of a new *process* than by the discovery of a new *substance*; and its theories are more immediately affected by the nature of a process of change than by any physical fact, such as the existence of a peculiar body or class of bodies. Thus it is that the method of isolating the anhydrous organic acids, has afforded evidence of a new view of the constitution of acids and salts.

A few words may serve to give an idea of the previous state of the question.

Compounds of oxygen-acids were supposed to consist of the anhydrous acid united with an oxide. Thus hydrated sulphuric acid was represented as containing the anhydrous group S O_3 , plus an atom of water H_2O ; and in the saturation of this hydrated

acid by a base such as potash, it was conceived that this oxide replaced the water. The existence of anhydrous sulphuric acid in an isolated state, and the fact that it so readily combines with water, was urged as an argument in favour of this theory; and the same holds good with phosphoric, carbonic, sulphurous, lactic, nitrous, and even (according to the recent discovery of Dessaignes) nitric acid.

However simple this view might appear and however satisfactory it might be in explaining those cases of combination for which it was specially intended, chemists soon became acquainted with bodies perfectly analogous in their general properties to the oxygen-acids, and producing by their action upon bases similar effects, but which, from the fact of their containing no oxygen, could not possibly be conceived as made up of water and an anhydrous-acid. For instance, hydrochloric acid was proved, both analytically and synthetically, to be composed of nothing but chlorine and hydrogen; and when it combines with potash, the hydrogen is found to leave the chlorine, whilst potassium takes its place.

Being desirous of simplifying as far as possible their views of these phenomena, and of extending the same explanation to all like cases, certain chemists were led to imagine a new mode of representing the constitution and reactions of oxygen-acids, which had the advantage of connecting the two classes of analogous reactions by the same theory. This consisted in conceiving, that in the formation of a hydrated acid, a compound radical is produced in combination with hydrogen; so that hydrated sulphuric acid is the hydrogen-compound of S O_4 , in the same way as hydrochloric acid is the hydrogen-compound of chlorine. There were many arguments in favour of this view, amongst which the most prominent was derived from the fact, that when a salt of the one class, as chloride of potassium, decomposes a salt of the other, as sulphate of silver, the result is exactly in conformity with what must occur on the supposition of the compound radical; and in like manner, the electrolytic decomposition of a sulphate moves the group S O_4 to the positive pole, where it either combines with a metal or undergoes decomposition.

One of the strongest arguments against the view that the oxygen-acids contain water, is afforded by the results of recent researches (especially of MM. Laurent and Gerhardt) on the atomic weight of acids. Those chemists have rendered more definite and exact than they had been before, our ideas on the distinctions between monobasic, bibasic, and tribasic acids, and have clearly established that the correct expression of the atom of nitric acid must be such as contains half as much hydrogen as is contained in one atom of water (inasmuch as water is bibasic, and nitric acid monobasic). Of course this proportion may be as well established

by doubling the atomic weight of water as by halving that of hydrated nitric acid; but either way it is clear that hydrated nitric acid cannot contain water.

Such was the position of the question, when an English chemist proved that the formation of ether from alcohol (which was considered chemically as the hydrate of ether,) does not consist in a separation of two already formed compounds, but in a substitution of hydrogen by the organic radical ethyl. A similar fact M. Gerhardt has proved respecting a great number of organic acids, by preparing bodies which stand to them in the same relation as ether does to alcohol.

The researches of M. Cahours had led to the discovery of a series of bodies necessary for Gerhardt's process. These were obtained by the action of pentachloride of phosphorus on various hydrated organic acids, and consisted of chlorine combined with the oxygenized radical of the acid. Thus from benzoic acid was prepared the chloride of benzoyl, $C_7 H_5 O Cl$, and the corresponding bodies, from cuminic, cinnamic, and various other acids. Gerhardt has since made by the same process the body $C_2 H_3 O Cl$, which is the chloride of the radical of acetic acid, called *othyl*. Now, on bringing any one of these chlorides in contact with the potassium-salt of the corresponding acid, the chemical force of combination between chlorine and potassium induced the decomposition.

These results can be most simply stated in the form adopted by M. Gerhardt the discoverer, which consists in comparing the composition of these bodies with that of water, from which they are formed by the substitution of one or both atoms of hydrogen by organic radicals.

Thus water being represented by the formula $\begin{smallmatrix} H \\ H \end{smallmatrix} O$, acetic acid is formed from it by the action of chloride of othyl $C_2 H_3 O Cl$, which forms $C_2 \begin{smallmatrix} H_3 \\ H \end{smallmatrix} O O + H Cl$ i. e. hydrated acetic acid and hydrochloric acid. If a second atom of chloride of othyl is made to act upon this acetic acid, or better upon the acetate of potash $C_2 H_3 O \begin{smallmatrix} O \\ K \end{smallmatrix}$, we get, besides chloride of potassium, a compound $C_2 \begin{smallmatrix} H_3 \\ H \end{smallmatrix} O O$ which when compared to the original type, may be considered as water having both its atoms of hydrogen replaced by the radical othyl, $C_2 H_3 O$. This compound is the anhydrous acetic acid which might be called the acetate of othyl, inasmuch as that radical has, in the formation of the compound, taken the place of the basic potassium in the acetate of potash.

In like manner, the anhydrous benzoic acid $C_7 \begin{smallmatrix} H_5 \\ H \end{smallmatrix} O O$ was made

by the action of the chloride of benzoil C_7H_5OCl on the benzoate of potash. It is a crystalline body, perfectly neutral to test paper, scarcely soluble in water, readily soluble in alcohol and ether. On continued boiling with water, it is converted into hydrated benzoic acid, one atom of the anhydride with one atom of water forming two atoms of the hydrated acid by an interchange of hydrogen and benzoil. Besides several of these anhydrous acids, Gerhardt has prepared some intermediate acids, analogous to the intermediate ethers, by combining two different radicals in the same group. Thus chloride of benzoil with cuminate of potash $C_{10}H_{11}O$ formed cuminate of benzoil or benzocuminic acid $C_{10}H_{11}O$ C_7H_5O ; and in like manner, several other intermediate acids were prepared.

In conclusion, to this very brief exposition of this important series of discoveries, the Lecturer alluded to a feature of the development of the human mind in scientific research, which is strikingly illustrated by the substance and form of these results, and of which instances are probably to be found in the history of many others. The explanation of the above reactions consists in a combination of two modes of reasoning, which were developed by different schools, and for many years were used independently of one another. Gerhardt, to whose researches and writings some important steps in the doctrine of types are owing, formerly believed the truths which he saw from that point of view to be incompatible with the idea of radicals, but he now joins those chemists who find in each of these notions a necessary and most natural complement to the other.

May we not hope that such may be the result in other cases of difference of opinion on scientific questions, which the progress of knowledge will shew to have been owing to the incompleteness and one-sidedness of each view, rather than to any thing absolutely erroneous in either?

[A. W. W.]

Friday, February 11.

THE DUKE OF NORTHUMBERLAND, K.G., F.R.S., President,
in the Chair.

JOHN TYNDALL, Esq., Ph. D., F.R.S.

*On the influence of Material Aggregation upon the manifestations
of Force.*

THERE are no two words with which we are more familiar than *matter* and *force*. The system of the universe embraces two things,—an object acted upon, and an agent by which it is acted upon;—the object we call matter, and the agent we call force. Matter, in certain aspects, may be regarded as the vehicle of force; thus the luminiferous ether is the vehicle or medium by which the pulsations of the sun are transmitted to our organs of vision. Or to take a plainer case; if we set a number of billiard balls in a row and impart a shock to one end of the series, in the direction of its length, we know what takes place; the last ball will fly away, the intervening balls having served for the transmission of the shock from one end of the series to the other. Or we might refer to the conduction of heat. If, for example, it be required to transmit heat from the fire to a point at some distance from the fire, this may be effected by means of a conducting body — by the poker for instance: thrusting one end of the poker into the fire it becomes heated, the heat makes its way through the mass, and finally manifests itself at the other end. Let us endeavour to get a distinct idea of what we here call heat; let us first picture it to ourselves as an agent apart from the mass of the conductor, making its way among the particles of the latter, jumping from atom to atom, and thus converting them into a kind of stepping-stones to assist its progress. It is a probable conclusion, even had we not a single experiment to support it, that the mode of transmission must, in some measure, depend upon the manner in which those little molecular stepping-stones are arranged. But we need not confine ourselves to the material theory of heat. Assuming the hypothesis which is now gaining ground, that heat, instead of being an agent apart from ordinary matter, consists in a motion of the material particles; the conclusion is equally probable that the transmission of the motion must be influenced by the manner in which the particles are arranged. Does experimental science furnish us with any corroboration of this inference? It does. More than twenty years ago MM. De la Rive and De Candolle proved that heat is transmitted through wood with a velocity almost twice as great along the fibre as across it. This result has been recently expanded, and it has been proved that this substance possesses three

axes of calorific conduction ; the first and greatest axis being parallel to the fibre ; the second axis perpendicular to the fibre and to the ligneous layers ; while the third axis, which marks the direction in which the greatest resistance is offered to the passage of the heat, is perpendicular to the fibre and parallel to the layers.

But it is the modification of the magnetic force by the peculiarities of aggregation, which forms the subject of the evening's lecture. What has been stated regarding heat applies with equal force to magnetism. The observed magnetic phenomena are of a composite character. The action of a magnetic mass is the resultant action of its molecules, and will be influenced by the manner in which they are aggregated. The fundamental phenomena of magnetism are too well known to render it necessary to dwell upon them for an instant. A small bar of iron was suspended in the magnetic field ; it set its length parallel to the line joining the poles. Should we be justified from this experiment in concluding that a magnetic mass will always set its longest dimension axial ? No. A second magnetic bar, equal in size to the former, was suspended between the poles ; it set its length at right angles to the line joining the poles. Whence this deportment ? We find the reason of it in the mechanical structure of the bar : it is composed of magnetic plates, transverse to its length : these plates set from pole to pole and hence the length of the bar equatorial. But let us proceed from this coarse experiment to one more delicate, where nature herself has imposed the conditions of aggregation. A plate taken from a mass of shale, picked up a few weeks ago in the coal district of Blackburn, was suspended between the poles ; although strongly magnetic it set its longest dimension at right angles to the line joining the poles. This deportment was at once explained by reference to the structure of the mass : it also, though apparently compact, was composed of layers transverse to its length ; these layers set from pole to pole and hence the length equatorial. Let us ascend to a case still more refined. A crystal of sulphate of nickel was suspended between the poles, and on exciting the magnet a certain determinate position was taken up by the crystal. The substance was magnetic, still its shortest dimension set from pole to pole. The crystal was removed from the magnetic field and the edge of a penknife placed along the line which set axial ; a slight pressure split the crystal and disclosed two beautiful surfaces of cleavage. The crystal could in this way be cloven into an indefinite number of magnetic layers ; these layers set from pole to pole and hence the longest dimension, which was perpendicular to the layers, equatorial. Comparing all these experiments,—ascending from the gross case where the laminæ were plates of iron stuck together by wax, to that in which they were crystalline, the inference appears unavoidable that the unanimity of deportment exhibited is the product of a common cause ; and that the results are due to the peculiarities of material aggregation.

The beautiful researches of Plücker in this domain of science are well known. Plücker's first experiment was made with a plate of tourmaline. Suspended in the magnetic field with the axis of the crystal vertical, it set its length from pole to pole, like an ordinary magnetic body. Suspended with the axis of the horizontal, on exciting the magnet, Plücker found to his astonishment that the largest dimension set equatorial. Let us see whether we cannot obtain this deportment otherwise. Suspending the piece of shale already made use of, so that its laminæ were horizontal, on exciting the magnet the longest horizontal dimension of the plate set axial: moving the point of suspension 90° so that the laminæ were vertical, on exciting the magnet the length of the plate set equatorial. In the magnetic field the deportment of the crystal was perfectly undistinguishable from that of the shale. But it may be retorted that tourmaline possesses no such laminæ as those possessed by the shale: true — nor is it necessary that it should do so. A number of plates, bars, and disks, formed artificially from magnetic dust, exhibited a deportment precisely similar to the tourmaline, — suspended from one point they set their lengths axial, suspended from another point the lengths set equatorial. Let us now turn to what may be called the complementary actions exhibited by diamagnetic bodies. A homogeneous diamagnetic bar sets its length equatorial. But bars were exhibited composed of transverse diamagnetic laminæ which set their lengths axial. This experiment is complementary to that of the shale, &c.; the magnetic laminæ set axial, the diamagnetic equatorial; and by attention to this the magnetic body is made to behave like a homogeneous diamagnetic body, and the diamagnetic body like a homogeneous magnetic body. Diamagnetic bars and disks were also examined, and a deportment precisely complementary to that of the magnetic bars and disks was exhibited. A magnetic disk set its thickness from pole to pole and consequently its horizontal diameter equatorial; a diamagnetic disk set its thickness equatorial and its horizontal diameter from pole to pole. Two bodies of the same exterior form and of the same colour, were suspended simultaneously in the fields of two electro-magnets, and both the latter were excited by the same current; the eye could detect no difference of deportment. Both bodies possessed the shape of calcareous spar, and both set the crystallographic axis equatorial. One body however was composed of wax, while the other was a true crystal. In the same way a crystal of carbonate of iron exhibited a deportment precisely the same as that of a model formed of magnetic dust. The explanation of these phenomena may be given in a few words. In the construction of the models, the magnetic or diamagnetic dust was formed into a kind of dough and pressed between two glass plates; the same process was applied to the wax; and it is a universal law, that in diamagnetic bodies the line along which the density of the mass has been increased by compression, sets equatorial,

and in magnetic bodies axial. A reference to this principle will instantly render plain all the experiments we have described. In those cases where the same artificial bar set at one time axial and at another time equatorial, the deportment depended on the circumstance whether the line of compression was vertical or horizontal. When vertical its directive power was annulled, and the action was determined by the exterior form of the body; but when horizontal its directive action came into play and determined the position of the mass. The magnetic bar, for example, suspended with its line of pressure vertical, set axial, but with its line of pressure horizontal, it set equatorial; for the pressure was exerted at right angles to its length. This action is so general that it is difficult to find a body so perfectly homogeneous as not to exhibit it in some degree. Ipecacuanha lozenges and Carlisle biscuits were suspended in the magnetic field and exhibited a most striking directive action. The materials in both cases were diamagnetic; but owing to the pressure exerted in their formation their largest horizontal dimensions set from pole to pole, the line of compression being equatorial.

Let us endeavour to arrive at the precise logical import of these experiments. Let us suppose that before ever a crystal had been suspended in the magnetic field, we were acquainted with the fact that a slight change of density in any direction is accompanied by such modifications of the magnetic force as those above described: — that we knew that flour, bran, soap, shale, magnetic dust, diamagnetic dust, &c., all exhibited this directive action, — that it is in fact a universal law of matter; and then let us imagine some fortunate experimenter hanging a crystal between the poles and observing a deportment in every respect similar. Would not the analogy of the case at once flash upon him? Would he not regard this deportment as a beautiful, but still special example of that all-pervading law with which he was previously acquainted. Would he not congratulate himself on the possibility thus opened to him of searching out the mysteries of crystalline structure, and rendering apparent to his mental eye the manner in which the molecules are aggregated together. He would never have assumed the existence of forces altogether new to account for the observed actions; much less would he have affirmed that they were wholly independent of magnetism or diamagnetism; for he would know beforehand that the modification of these forces by the peculiarities of aggregation was the exact thing calculated to produce the phenomena. But magne-crystalline action was discovered when its universality was unknown; and hence its discoverer was led to regard it as something unique. A great temptation lay in his way: years before, a magnet, now present, had twisted a ray of light, and thus suggested a connexion between light and magnetism. What wonder then if this unifying instinct, this yearning to find the mystic bond which unites these forces, this prediction of the human mind that all the forces of

nature are but branches of a common root, — what wonder, I say, if it jumped its bounds and cried “I have it!” too soon. For a long time the optic axis, and it alone, was chargeable with these phenomena, — phenomena which it was now hoped, there would be little difficulty in referring to their proper cause, and regarding as examples of the modification of force by the peculiarities of aggregation.

The Lecturer then pointed out the bearing of the described results upon the problem of the diurnal range of the magnetic needle. Professor Faraday had referred the matter to the modification of atmospheric magnetism by the sun’s rays. That an effect was produced here could not for a moment be doubted, but the precise extent of this effect was still an open question. The discovery of a decimal period by Lamont threw a great difficulty in the way of any theory which would refer the diurnal range to thermic action; and the difficulty was greatly increased by the observation of Col. Sabine, who connected Lamont’s discovery with that of Schwabe regarding the solar spots. But whatever the result of future enquiries as to the direct magnetism of the sun may be, no theory which proposes to exhaust the subject can afford to omit the mediate operation of the sun by his heat; not however confining it to the atmosphere, but extending it also to the earth’s solid crust. Let us look once more to our experiments. The line of greatest density is that of strongest magnetic power. The body operated upon by the magnet is itself a magnet, and it is an experimental fact, that it is a stronger magnet along the line of greater density than along any other line. If instead of increasing the density in one direction we increase it in all directions, we thereby augment the general magnetic power of the body. Anything therefore which tends to increase density increases magnetic power; and whatever diminishes density diminishes magnetic power also. Knowing this, the conclusion is inevitable, that the local action of the sun upon the earth’s crust must influence, in some degree, the resultant effect. The action here meant is wholly different from that hitherto speculated on, and which had reference to the generation of thermo-electric currents which affect the needle. The simple mechanical change of density is what is meant. It is a true cause, and no complete theory can omit taking it into account.

The Lecturer then proceeded to remark on the influence of geologic changes upon the earth as a magnet, and concluded as follows:

“This evening’s discourse is, in some measure, connected with this locality; and thinking thus, I am led to enquire wherein the true value of a scientific discovery consists? Not in its immediate results alone, but in the prospect which it opens to intellectual activity, in the hopes which it excites, in the vigour which it awakens. The discovery which led to the results brought before you to-night was of this character. *That* magnet was the physical birth-place of these results; and if they possess any value they are to be regarded as the returning crumbs of that bread which in 1846

was cast so liberally upon the waters. I rejoice, Ladies and Gentlemen, in the opportunity here afforded me of offering my tribute to the greatest worker of the age, and of laying some of the blossoms of that prolific tree which he planted, at the feet of the great discoverer of diamagnetism."

[J. T.]

Friday, February 18.

THE DUKE OF NORTHUMBERLAND, K.G., F.R.S., President,
in the Chair.

G. G. STOKES, M.A., F.R.S., Lucasian Professor, Cambridge.

*On the Change of Refrangibility of Light, and the exhibition thereby
of the Chemical Rays.*

BEFORE proceeding to the more immediate subject of the Lecture, it was necessary to refer to certain discoveries of Sir John Herschel and Sir David Brewster, more especially as it was the discovery by the former of these philosophers of the epipolic dispersion of light, and of the peculiar analysis of light which accompanies the phenomenon, that led to the researches respecting the change of refrangibility.

When a weak acid solution of quinine is prepared, by dissolving, suppose, one part of the commercial disulphate in 200 parts of water acidulated with sulphuric acid, a fluid is obtained which appears colourless and transparent when viewed by transmitted light, but which exhibits nevertheless in certain aspects a peculiar sky-blue colour. This colour of course had frequently been noticed; but it is to Sir John Herschel that we owe the first analysis of the phenomenon.* He found that the blue light emanates in all directions from a very thin stratum of fluid adjacent to the surface, (whether it be the free surface or the surface of contact of the fluid with the containing glass vessel,) by which the incident rays enter the fluid. His experiments clearly shew that what here takes place is not a mere *subdivision* of light into a portion which is dispersed and a portion which passes on, but an actual *analysis*. For after the rays have once passed* through the stratum from which the blue dispersed light comes, they are deprived of the power of producing the

* Philosophical Transactions for 1845, p. 143.

same effect; that is, they do not exhibit any blue stratum when they are incident a second time on a solution of quinine. To express the modification which the transmitted light had undergone, the further nature of which did not at the time appear, Sir John Herschel made use of the term "epipolized."

Sir David Brewster had several years before discovered a remarkable phenomenon in an alcoholic solution of the green colouring matter of leaves, or, as it is called by chemists, chlorophyll. This fluid when of moderate strength and viewed across a moderate thickness is of a fine emerald green colour; but Sir David Brewster found that when a bright pencil of rays, formed by condensing the sun's light by a lens, was admitted into the fluid, the path of the rays was marked by a *bright beam of a blood red colour*.^{*} This singular phenomenon he has designated *internal dispersion*. He supposed it to be due to suspended particles which reflected a red light, and conceived that it might be imitated by a fluid holding in suspension an excessively fine coloured precipitate. A similar phenomenon was observed by him in a great many other solutions, and in some solids; and in a paper read before the Royal Society of Edinburgh in 1846 he has entered fully into the subject.[†] In consequence of Sir John Herschel's papers, which had just appeared, he was led to examine a solution of sulphate of quinine; and he concluded from his observations that the "epipolic" dispersion of light exhibited by this fluid was only a particular instance of internal dispersion, distinguished by the extraordinary rapidity with which the rays capable of dispersion were dispersed.

The Lecturer stated, that, having had his attention called some time ago to Sir John Herschel's papers, he had no sooner repeated some of the experiments than he felt an extreme interest in the phenomenon. The reality of the epipolic analysis of light was at once evident from the experiments; and he felt confident that certain theoretical views respecting the nature of light had only to be followed fearlessly into their legitimate consequences, in order to explain the real nature of epipolized light.

The exhibition of a richly coloured beam of light in a perfectly clear fluid, when the observation is conducted in the manner of Sir David Brewster, seemed to point to the dispersions exhibited by the solutions of quinine and chlorophyll as one and the same phenomenon. The latter fluid, as has been already stated, disperses light of a blood red colour. When the transmitted light is subjected to prismatic analysis, there is found a remarkably intense band of absorption in the red, besides certain other absorption bands, of less intensity, in other parts of the spectrum. Nothing at first seemed more likely than that, in consequence of some action of the ultimate molecules of the medium, the incident rays belonging to the absorp-

^{*} Edinburgh Phil. Trans. Vol. XII. p. 542.

[†] Vol. XVI. Part 2, and Phil. Mag. June, 1846.

tion band in the red, withdrawn, as they certainly were, from the incident beam, were given out in all directions, instead of being absorbed in the manner usual in coloured media. It might be supposed that the incident vibrations of the luminiferous ether generated synchronous vibrations in the ultimate molecules, and were thereby exhausted, and that the molecules in turn became centres of disturbance to the ether. The general analogy between the phenomena exhibited by the solutions of chlorophyll and of quinine would lead to the expectation of absorption bands in the light transmitted by the latter. If these bands were but narrow, the light belonging to them might not be missed in the transmitted beam, unless it were specially looked for; and the beam might be thus "epipolized," without, to ordinary inspection, being changed in its properties in any other respect. But on subjecting the light to prismatic analysis, first with the naked eye, and then with a magnifying power, no absorption bands were perceived.

A little further reflection shewed that even the supposition of the existence of these bands would not alone account for the phenomenon. For the rays producing the dispersed light, (if we confine our attention to the thin stratum in which the main part of the dispersion takes place,) are exhausted by the time the incident light has traversed a stratum the fiftieth of an inch thick, or thereabouts, whereas the dispersed rays traverse the fluid with perfect freedom. This indicates a *difference of nature* between the blue-producing rays and the blue rays produced. Now, as the Lecturer stated, he felt very great confidence in the principle that the nature of light is completely defined by specifying its refrangibility and its state as to polarization. The difference of nature, then, indicated by the phenomenon, must be referred to a difference in one or other of these two respects. At first he took for granted that there could be no change of refrangibility. The refrangibility of light had hitherto been regarded as an attribute absolutely invariable.* To suppose that it had changed would, on the undulatory theory, be equivalent to supposing that periodic vibrations of one period could give rise to periodic vibrations of a different period, a supposition presenting no small mechanical difficulty. But the hypotheses which he was *obliged* to form on adopting the other alternative, namely, that the difference of nature had to do with the state of polarization, were so artificial as to constitute a theory which appeared utterly extravagant. He was thus led to contemplate the possibility of a change of refrangibility. No sooner had he dwelt

* It is true that the phenomenon of phosphorescence is in a certain sense an exception; but the effect is in this case a work of time, which seems at once to remove it from all the ordinary phenomena of light, which, as far as sense can judge, take place instantaneously. It is true that there now appears a close analogy in many respects between true internal dispersion and phosphorescence. But while the nature of epipolized light remained yet unexplained, there was nothing in the former phenomenon to point to the latter.

in his mind on this supposition, than the mystery respecting the nature of epipolized light vanished; all the parts of the phenomenon fell naturally into their places. So simple did the whole explanation become, when once the fundamental hypothesis was admitted, that he could not help feeling strongly impressed that it would turn out to be true. Its truth or fallacy was a question easily to be decided by experiment; the experiments were performed, and resulted in its complete establishment.

The Lecturer then described what may be regarded as the fundamental experiment. A beam of sunlight was reflected horizontally through a vertical slit into a darkened room, and a pure spectrum was formed in the usual manner, namely, by transmitting the light through a prism at the distance of several feet from the slit, and then through a lens close to the prism. In the actual experiment, two or three prisms were used, to produce a greater angular separation of the colours. Instead of a screen, there was placed at the focus of the lens a vessel containing a solution of sulphate of quinine. It was found that the red, orange, &c., in fact, nearly the whole of the visible rays, passed through the fluid as if it had been mere water. But on arriving about the middle of the violet, the path of the rays within the fluid was marked by a sky-blue light, which emanated in all directions from the fluid, as if the medium had been self-luminous. This blue light continued throughout the region of the violet, and far beyond, in the region of the invisible rays. The posterior surface of the luminous portion of the fluid marked the distance to which the incident rays were able to penetrate into the medium before they were exhausted. This distance, which at first exceeded the diameter of the vessel, decreased with great rapidity, so that in the greater part of the invisible region it amounted to only a very small fraction of an inch. The fixed lines of the extreme violet, and of the more refrangible invisible rays, were exhibited by dark planes interrupting the dispersed light. When a small portion of the incident spectrum was isolated, by stopping the rest by a screen, and the corresponding beam of blue dispersed light was refracted sideways by a prism held to the eye, it was found to consist of light having various degrees of refrangibility, with colour corresponding, the more refrangible rays being more abundant than the less refrangible. The nature of epipolized light is now evident; it is nothing but light from which the highly refrangible invisible rays have been withdrawn by transmitting it through a solution of quinine, and does not differ from light from which those rays have been withdrawn by any other means.

The fundamental experiment, excepting that part of it which relates to the analysis of the dispersed light, was then exhibited by means of the powerful voltaic battery belonging to the Institution, which was applied to the combustion of metals. The rays emanating from the voltaic arc were applied to form a pure spectrum, which was received on a slab of glass coloured by peroxide of ura-

nium, a medium which possesses properties similar to those of a solution of sulphate of quinine in a still more eminent degree.

The difference of nature of the illumination produced by a change of refrangibility, or "true internal dispersion," from that due to the mere scattering of light, may be shown in a very instructive form by placing paper washed with sulphate of quinine, or a screen of similar properties, so as to receive a long narrow horizontal spectrum, and refracting this upwards by a prism held to the eye. Were the luminous band formed on the paper due merely to the scattering of the incident rays, it ought of course to be thrown obliquely upwards; whereas it is actually decomposed by the prism into two bands, one ascending obliquely, and consisting of the usual colours of the spectrum in their natural order, the other running horizontally, and extending far beyond the more refrangible end of the former. Whatever be the screen, the horizontal band is always situated below the oblique, since there appears to be no exception to the law, that when the refrangibility of light is changed in this manner it is *always lowered*.

The general appearance of some highly "sensitive" media in the invisible rays was then exhibited by means of the flame of sulphur burning in oxygen, a source of these rays which Dr. Faraday, to whose valuable assistance the Lecturer was much indebted, had in some preliminary trials found very efficacious. The chief media used were articles made of glass coloured by uranium, and solutions of quinine, of horse-chestnut bark, and of the seeds of the datura stramonium. A tall cylindrical jar filled with water showed nothing remarkable; but when a solution of horse-chestnut bark was poured in, the descending fluid was strongly luminous. The experiment was varied by means of white paper on which words had been written with a pretty strong solution of sulphate of quinine, an alcoholic solution of the seeds of the datura stramonium, and a purified aqueous solution of horse-chestnut bark. By gas-light the letters were invisible; but by the sulphur light, especially when it had been transmitted through a blue glass, which transmits a much larger proportion of the invisible than of the visible rays, the letters appeared luminous, on a comparatively dark ground. A glass vessel containing a thin sheet of a very weak solution of chromate of potash allowed the letters to be seen as well, or very nearly as well as before, when it was interposed between the eye and the paper; but when it was interposed between the flame and the paper the letters wholly disappeared,—the medium being opaque with respect to the rays which caused the letters to be luminous, but transparent with respect to the rays which they emitted.

It was then remarked what facilities are thus afforded for the study of the invisible rays. When a pure spectrum is once formed, it is as easy to determine the mode of absorption of an absorbing medium with respect to the invisible, as with respect to the visible rays. It is sufficient to interpose the medium in the path of the incident

rays, and to notice the effect. Again, the effect of various flames and other sources of light on solutions of quinine, and on similar media, indicates the richness or poverty of those sources with respect to the highly refrangible invisible rays. Thus, the flames of alcohol, of hydrogen, &c., of which the illuminating power is so feeble, were found to be very rich in invisible rays. This was still more the case with a small electric spark, while the spark from a Leyden jar was found to abound in rays of excessively high refrangibility. These highly refrangible rays were stopped by glass, but passed freely through quartz. These results, and others leading to the same conclusion, had induced the Lecturer to order a complete train of quartz. A considerable portion of this was finished before the end of last August, and was applied to the examination of the solar spectrum. A spectrum was then obtained extending beyond the visible spectrum, that is, beyond the extreme violet, to a distance at least double that of the formerly known chemical spectrum. This new region was filled with fixed lines like the regions previously known.

But a spectrum far surpassing this was obtained with the powerful electrical apparatus belonging to the Institution. The voltaic arc from metallic points furnished a spectrum no less than *six or eight times* as long as the visible spectrum. This was in fact the spectrum which had already been exhibited in connexion with the fundamental experiment. The prisms and lens which the Lecturer had been employing in forming the spectrum were actually made of quartz. The spectrum thus obtained was filled from end to end with bright bands. When a piece of glass was interposed in the path of the incident rays, the length of the spectrum was reduced to a small fraction of what it had been, all the more refracted part being cut away. A strong discharge of a Leyden jar had been found to give a spectrum at least as long as the former, but not, like it, consisting of nothing but isolated bright bands.

The Lecturer then explained the grounds on which he concluded that the end of the solar spectrum on the more refrangible side had actually been reached, no obstacle existing to the exhibition of rays still more refrangible if such were present. He stated also that during the winter, even when the sun shone clearly, it was not possible to see so far as before. As spring advanced he found the light continually improving, but still he was not able to see so far as he had seen at the end of August. It was plain that the earth's atmosphere was by no means transparent with respect to the most refrangible of the rays belonging to the solar spectrum.

In conclusion, there was exhibited the effect of the invisible rays coming from a succession of sparks from the prime conductor of a large electrifying machine, in illuminating a slab of glass coloured by uranium.

[G. G. S.]

Friday, May 27.

RIGHT HON. BARON PARKE, Vice-President, in the Chair.

B. C. BRODIE, Esq., F.R.S.

On the formation of Hydrogen and its Homologues.

IN, what is termed, mineral chemistry, chemical substances are classified according to the different nature of the elements of which they consist. But in organic chemistry this distinction is no longer available. Organic substances were formerly defined as triple compounds of carbon, hydrogen, and oxygen, and this, with the statement of the relative proportion of these elements in any given compound, was all that was attempted to be made out as to its constitution. But this class of bodies is more numerous, possibly, than all the other chemical substances taken together, with which we are acquainted, and some further distinction was necessary for the purposes of science. The sagacity of certain chemists at length discovered a relation which was capable of becoming the basis of a truly rational and natural classification. It was perceived that in the long series of chemical changes of which these bodies were susceptible, the whole of the substance did not change, and that, in these combinations, certain *groups* of elements had the same persistent character and fulfilled the same chemical function as the simple elements themselves in other bodies. These constant groups have been named *radicals*. Among these those hydrocarbons termed the homologues of hydrogen are of special interest. Ethyl, a groupe consisting of two atoms of carbon and five of hydrogen, $C_2 H_5$, is one of these bodies. Assuming water as two atoms of hydrogen and one of oxygen, $H H O$, alcohol is composed of one atom of ethyl, one of hydrogen, and one atom of oxygen, $(C_2 H_5) H O$. Hydriodic acid, the iodide of hydrogen, consists of an atom of hydrogen combined with an atom of iodine, $H I$. The iodide of ethyl consists of an atom of ethyl combined with an atom of iodine, $(C_2 H_5) I$. It is from these and other like analogies between hydrogen and ethyl, that the idea arose of the similarity in their chemical function.

Certain chemists however conceived these views to be mere fanciful speculations. Their principal objection, reasonable or not, was that this ethyl was a purely ideal substance. From hydrochloric acid, or from water, we readily procure hydrogen. We separate metals from their combinations; but ethyl could not thus

be obtained, and there was a point where it seemed that this analogy failed. Frankland however has silenced this objection in the most satisfactory manner, namely by procuring and isolating this ethyl.

He prepared it by a modification of the form of experiment by which hydrogen itself is prepared. He placed together zinc and iodide of ethyl in tubes hermetically sealed, and heated them considerably above the boiling point of water. On opening the tubes the ethyl escapes as a colourless combustible gas. There is only one property of ethyl on which I need dwell, its weight—it is about twice the weight of air.

Ethyl, however, when procured, did not realize all the anticipations formed of it, and there was one very important difference between the actual and the anticipated ethyl. It was supposed that when zinc acts upon iodide of hydrogen it takes away (so to say) the iodine, and the hydrogen becomes, what is termed, *free*, and the same with ethyl. On this view ethyl would have a certain atomic constitution, $C_2 H_5$. Now there is much reason to believe, that in the gaseous form the molecules of all bodies occupy the same space, whether this molecule consist of two only, or, as may be the case, of one hundred atoms. Hence to ascertain of how many atoms the molecule of a substance consists, we have simply to compare its weight in the gaseous form, with that of some other gas of which the molecule is already determined. When this experiment was made with ethyl, it was found to be just twice as heavy as it should be; that is to say, the space which should have contained two atoms of carbon and five of hydrogen, was found to contain just twice that quantity, or $C_4 H_{10}$.

Some chemists considered that ethyl was an exception to the general rule, and that the molecule of ethyl only occupied half the space of the molecule of other bodies, so that the same space which contained one molecule of water truly contained two molecules of ethyl. This however is evidently but an arbitrary assumption to meet the case. Others said that, after all, the true ethyl remained yet to be discovered, and that this body was not it, but a hydrocarbon isomeric with it, for that the real ethyl would have only half the density of this body.

There is, however, a third view, on which the ethyl of theory is also the ethyl of fact. On a former occasion I shewed reasons for believing that the elements are in a certain sense compound molecular groupings, consisting of two or more atoms, which (in the present state of our knowledge) we must regard as similar, united to form a compound molecule. On this idea the gas hydrogen is represented, not by the symbol H, but as H H : and ethyl the analogue of hydrogen would also consist of a double atom, and be represented not as $C_2 H_5$, but as $C_2 H_5 C_2 H_5$.

The old view, however, had always a certain advantage over this, in the clear and consistent account which it gave of the mode of formation of hydrogen. How is it, it may be asked, and by what

process, that this compound atom of hydrogen is formed? The answer is by no means obvious. Indeed the investigation of the nature of the process by which ethyl was formed, alone gave the key to its solution.

Ethyl is not, in truth, made by the direct action of zinc upon the iodide of ethyl; but by the intervention of another body, which belongs to the class of, what I may term, fugitive or evanescent combinations, and which is made and decomposed again in the course of the experiment. This body is zinc-ethyl. The molecule of zinc, consisting of two atoms Zn Zn , splits into two parts. One atom, Zn , combines with the iodine of the iodide of ethyl, $\text{C}_2 \text{H}_5 \text{I}$, to form iodide of zinc, Zn I , while the other atom at the same moment combines with the ethyl, forming zinc-ethyl, $\text{Zn C}_2 \text{H}_5$.

The mode of action of zinc-ethyl upon iodide of ethyl is perfectly analogous to its action upon water. In contact with water, H H O , it immediately decomposes, forming hydrated oxide of zinc, Zn H O , and hydride of ethyl $\text{C}_2 \text{H}_5 \text{H}$. This hydride of ethyl has hardly more than half the density of the ethyl gas. In the same space in which, in the other case, are contained two heavy atoms of ethyl are here contained one heavy atom of ethyl and one light atom of hydrogen. Now the zinc-ethyl with the iodide of ethyl decomposes in a perfectly similar manner, forming iodide of zinc, Zn I , and ethyl gas $\text{C}_2 \text{H}_5$. That this is truly the mode of the formation of the ethyl is proved by the fact, that by careful modification of the experiment, it is possible to break up this process of the formation of the ethyl into the two factors (so to say) of which it consists.* At a low temperature the zinc-ethyl alone is formed. At a higher temperature the zinc-ethyl disappears and the ethyl is produced.

It is evident that the formation of the compound molecule of hydrogen H H , must be a very different physical event to the formation of the single atom of hydrogen H , if such could exist. The ordinary hypothesis of the 'liberation' of hydrogen gives us no conception of its nature: we need some other explanation.

It seems to me probable that when zinc acts on hydrochloric acid and water, there are, as in the case of ethyl, two steps in the process; the first, the formation of a zinc-hydrogen, Zn H , the second, the action of this zinc-hydrogen on the water with the formation of hydrated oxide of zinc, Zn H O , and hydrogen gas H H . There are various arguments in favour of this view. First, it explains the result, which the other hypothesis does not. Secondly, the analogy of ethyl compels us to it. It is not probable that bodies so similar, in other respects, are dissimilar in the mode of their formation. Thirdly, there is, at least, one experiment in which we are absolutely able to analyse the process of the formation of hydrogen and to prove that it does take place in this manner.

* See Quarterly Journal of the Chemical Society, Vol. III. p. 405.

This remarkable experiment is the formation of hydrogen by the decomposition of hypophosphorous acid by copper salts.* Hypophosphorous acid is, like zinc, what is termed a reducing agent. It precipitates certain metals from their solutions, and by a process of disoxydation decomposes alkalies with the formation of phosphorous acid and hydrogen. If this hypophosphorous acid be *boiled* with the copper salt, nothing is perceived but the formation of metallic copper and hydrogen gas; but if the solution be gradually heated, and the action arrested at a certain point, it can be shewn that this formation of hydrogen is preceded by the formation of a combination of hydrogen and copper, Cu_2H , analogous to zinc-ethyl. The part which this bears in the formation of the hydrogen is distinctly shewn by the action of acids, hydrochloric acid, HCl , for example, upon it. This acid, which does not act upon metallic copper, immediately decomposes this body, forming protochloride of copper, Cu_2Cl , and hydrogen HH .

This hydride of copper has only a very ephemeral existence. It is decomposed very nearly at the same temperature at which it is produced, and its formation, for this reason, had long been overlooked by chemists. We can hence readily comprehend that other combinations of this class may take place in the case of which the temperature of formation and of decomposition may either coincide, or so closely approximate to each other, that it may ever be impossible to isolate the substance produced. This is probably the case in the action of zinc.

[B. C. B.]

* See Annales de Chimie, III. Serie, tome XI. p. 250.

Friday, June 10.

THE DUKE OF NORTHUMBERLAND, K.G., F.R.S., President,
in the Chair.

PROFESSOR FARADAY,

MM. Boussingault, Frémy, Becquerel, &c. on Oxygen.

THE object of the speaker was to bring before the Members, in the first place, M. Boussingault's endeavours to procure pure oxygen from the atmosphere in large quantities; so that being stored up in gasometers it might afterwards be applied to the many practical and useful purposes which suggest themselves at once, or which may hereafter be developed. The principle of the process is to heat baryta in close vessels and peroxidize it by the passage of a current of air; and afterwards by the application of the same heat, and a current of steam (with the same vessels) to evolve the extra portion of oxygen, and receive it in fitly adjusted gasometers: then the hydrated baryta so produced is dehydrated by a current of air passed over it at a somewhat higher temperature, and finally oxidized to excess by the continuance of the current and a lower temperature:—and thus the process recurs again and again. The causes of failure in the progress of the investigation were described as detailed by M. Boussingault; the peculiar action of water illustrated; the reason why a mixture of baryta and lime, rather than pure baryta, should be used, was given; and the various other points in the *Mémoire* of M. Boussingault* noticed in turn. That philosopher now prepares the oxygen for his laboratory use by the baryta process.

The next subject consisted of the recent researches of MM. Frémy and E. Becquerel, on the influence of the electric spark in converting pure dry oxygen into ozone. The electric discharge from different sources produces this effect, but the high intensity spark of the electric machine is that best fitted for the purpose. When the spark contains the same electricity, its effect is proportionate to its length; for at two places of discharge in the same circuit, but with intervals of 1 and 2, the effect in producing ozone is as 1 and 2 also. A spark can act by *induction*; for, when it passes on the *outside* a glass tube containing within dry oxygen, and hermetically

* *Annales de Chimie*, 1852, xxxv. p. 1.

sealed, the oxygen is partly converted into ozone. Using tubes of oxygen, which either stood over a solution of iodide of potassium or, being hermetically sealed, contained the metal silver, the oxygen converted into ozone was absorbed; and the conversion of the *whole* of a given quantity of oxygen into ozone could be thus established. The effect for each spark is but small; 500,000 discharges were required to convert the oxygen in a tube about 7 inches long and 0.2 in diameter into ozone. For the details of this research, see the *Annales de Chimie*, 1852, xxxv. 62.

Mr. Faraday then referred briefly to the recent views of Schönbein respecting the probable existence of part of the oxygen in oxygen compounds in the ozone state. Thus of the peroxide of iron, the third oxygen is considered by him as existing in the state of ozone; and of the oxygen in pernitrous acid, half, or the two latter proportions added when the red gas is formed from oxygen and nitrous gas, are supposed to be in the same state. Hence the peculiar chemical action of these bodies; which seems not to be accounted for by the idea of a bare adhesion of the last oxygen, inasmuch as a red heat cannot separate the third oxygen from the peroxide of iron; and hence also, according to M. Schönbein, certain effects of change of colour by heat, and certain other actions connected with magnetism, &c.

[M. F.]

Friday, January 20.

Right Hon. BARON PARKE, Vice-President, in the Chair.

PROFESSOR FARADAY, D.C.L., F.R.S.

On Electric Induction — Associated cases of current and static effects.

CERTAIN phenomena that have presented themselves in the course of the extraordinary expansion which the works of the Electric Telegraph Company have undergone, appeared to me to offer remarkable illustrations of some fundamental principles of Electricity, and strong confirmation of the truthfulness of the view which I put forth sixteen years ago, respecting the mutually dependent nature of induction, conduction, and insulation (Experimental Researches, 1318, &c.). I am deeply indebted to the Company; to the Gutta Percha works, and to Mr. Latimer Clarke, for the facts; and also for the opportunity both of seeing and shewing them well.

Copper wire is perfectly covered with gutta percha at the Company's works, the metal and the covering being in every part regular and concentric. The covered wire is usually made into half mile lengths, the necessary junctions being effected by twisting or binding, and ultimately, soldering; after which the place is covered with fine gutta percha, in such a manner as to make the coating as perfect there as elsewhere: the perfection of the whole operation is finally tried in the following striking manner, by Mr. Statham, the manager of the works. The half mile coils are suspended from the sides of barges floating in a canal, so that the coils are immersed in the water whilst the two ends of each coil rise into the air: as many as 200 coils are thus immersed at once, and when their ends are connected in series, one great length of 100 miles of submerged wire is produced, the two extremities of which can be brought into a room for experiment. An insulated voltaic battery of many pairs of zinc and copper, with dilute sulphuric acid, has one end connected with the earth and the other, through a galvanometer, with either end of the submerged wire. Neglecting the first effect, but continuing the contact, it is evident that the battery current can take advantage of the whole accumulated conduction or defective insulation in the 100 miles of gutta percha on the wire, and that whatever portion of electricity passes through to the water will be shewn by the galvanometer. Now the battery is made one of

intensity, in order to raise the character of the proof, and the galvanometer employed is of considerable delicacy ; yet so high is the insulation that the deflection is not more than 5° . As another test of the perfect state of the wire, when the two ends of the battery are connected with the two ends of the wire, there is a powerful current of electricity shewn by a much coarser instrument ; but when any one junction in the course of the 100 miles is separated, the current is stopped, and the leak or deficiency of insulation rendered as small as before. The perfection and condition of the wire may be judged of by these facts.

The 100 miles, by means of which I saw the phenomena, were thus good as to insulation. The copper wire was $\frac{1}{16}$ of an inch in diameter : — the covered wire was $\frac{4}{8}$; some was a little less, being $\frac{7}{32}$ in diameter : — the gutta percha on the metal may therefore be considered as 0.1 of an inch in thickness. 100 miles of like covered wire in coils were heaped up on the floor of a dry warehouse and connected in one series, for comparison with that under water.

Consider now an insulated battery of 360 pairs of plates (4×3 inches) having one extremity in contact with the earth, the water wire with both its insulated ends in the room, and a good earth discharge wire ready for the requisite communications : — when the free battery end was placed in contact with the water wire and then removed, and, afterwards, a person touching the earth discharge touched also the wire, he received a powerful shock. The shock was rather that of a voltaic than of a Leyden battery : it occupied *time*, and by quick tapping touches could be divided into numerous small shocks : I obtained as many as 40 sensible shocks from one charge of the wire. If *time* were allowed to intervene between the charge and discharge of the wire, the shock was less : but it was sensible after 2, 3, or 4 minutes, or even a longer period.

When, after the wire had been in contact with the battery, it was placed in contact with a Statham's fuze, it ignited the fuze (or even 6 fuzes in succession) vividly : — it could ignite the fuze 3 or 4 seconds after separation from the battery. When, having been in contact with the battery, it was separated and placed in contact with a galvanometer, it affected the instrument very powerfully : — it acted on it, though less powerfully, after the lapse of 4 or 5 minutes, and even affected it sensibly 20 or 30 minutes after it had been separated from the battery. When the insulated galvanometer was permanently attached to the end of the water wire, and the battery pole was brought in contact with the free end of the instrument, it was most instructive to see the great rush of electricity into the wire ; yet after that was over, though the contact was continued, the deflection was not more than 5° , so high was the insulation. Then separating the battery from the galvanometer, and touching the latter with the earth wire, it was just as striking to see the electricity rush out of the wire, holding for a time the

magnet of the instrument in the reverse direction to that due to the ingress or charge.

These effects were produced equally well with either pole of the battery or with either end of the wire; and whether the electric condition was conferred and withdrawn at the same end or at the opposite ends of the 100 miles, made no difference in the results. An intensity battery was required, for reasons which will be very evident in the sequel. That employed was able to decompose only a very small quantity of water in a given time. A Grove's battery of 8 or 10 pair of plates, which would have far surpassed it in this respect, would have had scarcely a sensible power in affecting the wire.

When the 100 miles of wire in the air were experimented with in like manner, not the slightest signs of any of these effects were produced. There is reason, from principle, to believe that an infinitesimal result is obtainable, but as compared to the water wire the action was nothing. Yet the wire was equally well and better insulated, and as regarded a constant current, it was an equally good conductor. This point was ascertained, by attaching the end of the water wire to one galvanometer, and the end of the air wire to another like instrument; the two other ends of the wires were fastened together, and to the earth contact; the two free galvanometer ends were fastened together, and to the free pole of the battery: in this manner the current was divided between the air and water wires, but the galvanometers were affected to precisely the same amount. To make the result more certain, these instruments were changed one for the other, but the deviations were still alike: so that the two wires conducted with equal facility.

The cause of the first results is, upon consideration, evident enough. In consequence of the perfection of the workmanship, a Leyden arrangement is produced upon a large scale: the copper wire becomes charged statically with that electricity which the pole of the battery connected with it can supply;* it acts by induction through the gutta percha (without which induction it could not itself become charged, Exp. Res. 1177), producing the opposite state on the surface of the water touching the gutta percha, which forms the outer coating of this curious arrangement. The gutta percha across which the induction occurs, is only 0.1 of an inch thick, and the extent of the coating is enormous. The surface of the copper wire is nearly 8300 square feet, and the surface of the outer coating of water is four times that amount, or 33000 square feet. Hence, the striking character of the results. The intensity of the static charge acquired is only equal to the intensity at the pole of the battery whence it is derived; but its quantity is enormous, because of the immense extent of the Leyden

* Davy, Elements of Chemical Philosophy, p. 154.

arrangement; and hence when the wire is separated from the battery and the charge employed, it has all the powers of a considerable voltaic current, and gives results which the best ordinary electric machines and Leyden arrangements cannot as yet approach.

That the air wire produces none of these effects is simply because there is no outer coating correspondent to the water, or only one so far removed as to allow of no sensible induction, and therefore the inner wire cannot become charged. In the air wire of the warehouse, the floor, walls, and ceiling of the place constituted the outer coating, and this was at a considerable distance; and in any case could only affect the outside portions of the coils of wire. I understand that 100 miles of wire stretched in a line through the air, so as to have its whole extent opposed to earth, is equally inefficient in shewing the effects, and there it must be the distance of the inductric and inducteous surfaces (1483), combined with the lower specific inductive capacity of air, as compared with gutta percha, which causes the negative result. The phenomena altogether offer a beautiful case of the identity of static and dynamic electricity. The whole power of a considerable battery may in this way be worked off in separate portions, and measured out in units of static force, and yet be employed afterwards for any or every purpose of voltaic electricity.

I now proceed to further consequences of associated static and dynamic effects. Wires covered with gutta percha, and then inclosed in tubes of lead or of iron, or buried in the earth, or sunk in the sea, exhibit the same phenomena as those described; the like static inductive action being in all these cases permitted by the conditions. Such subterraneous wires exist between London and Manchester, and when they are all connected together so as to make one series, offer above 1500 miles; which, as the duplications return to London, can be observed by one experimenter at intervals of about 400 miles, by the introduction of galvanometers at these returns. This wire, or the half, or fourth of it, presented all the phenomena already described; the only difference was, that as the insulation was not so perfect, the charged condition fell more rapidly. Consider 750 miles of the wire in one length, a galvanometer *a* being at the beginning of the wire, a second galvanometer *b* in the middle, and a third *c* at the end:—these three galvanometers being in the room with the experimenter, and the third *c* perfectly connected with the earth. On bringing the pole of the battery into contact with the wire through the galvanometer *a*, that instrument was instantly affected; after a sensible time *b* was affected, and after a still longer time *c*: when the whole 1500 miles were included, it required two seconds for the electric stream to reach the last instrument. Again;—all the instruments being deflected, (of course not equally because of the electric leakage along the line,) if the battery were cut off at *a*, that instrument instantly fell

to zero; but *b* did not fall until a little while after; and *c* only after a still longer interval;—a current flowing on to the end of the wire whilst there was none flowing in at the beginning. Again; by a short touch of the battery pole against *a*, it could be deflected and could fall back into its neutral condition, before the electric power had reached *b*; which in its turn would be for an instant affected, and then left neutral before the power had reached *c*; a wave of force having been sent into the wire which gradually travelled along it, and made itself evident at successive intervals of time, in different parts of the wire. It was even possible, by adjusted touches of the battery, to have two simultaneous waves in the wire, following each other, so that at the same moment that *c* was affected by the first wave, *a* or *b* was affected by the second; and there is no doubt that by the multiplication of instruments and close attention, four or five waves might be obtained at once.

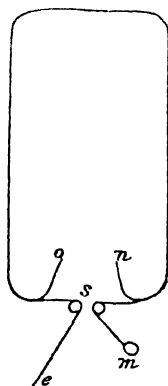
If after making and breaking battery contact at *a*, *a* be immediately connected with the earth, then additional interesting effects occur. Part of the electricity which is in the wire will return, and passing through *a* will deflect it in the reverse direction; so that currents will flow out of both extremities of the wire in opposite directions, whilst no current is going into it from any source. Or if *a* be quickly put to the battery and then to the earth, it will shew a current first entering into the wire, and then returning out of the wire at the same place; no sensible part of it ever travelling on to *b* or *c*.

When an air wire of equal extent is experimented with in like manner, no such effects as these are perceived: or if, guided by principle, the arrangements are such as to be searching, they are perceived only in a very slight degree, and disappear in comparison with the former gross results. The effect at the end of the very long air wire (or *c*) is in the smallest degree behind the effect at galvanometer *a*; and the accumulation of a charge in the wire is not sensible.

All these results as to *time*, &c. evidently depend upon the same condition as that which produced the former effect of static charge, namely, *lateral induction*; and are necessary consequences of the principles of conduction, insulation, and induction, three terms which in their meaning are inseparable from each other (Exp. Res. 1320, 1326,* 1338, 1561, &c.). If we put a plate of shell lac upon

* 1326. All these considerations impress my mind strongly with the conviction, that insulation and ordinary conduction cannot be properly separated when we are examining into their nature: that is, into the general law or laws under which their phenomena are produced. They appear to me to consist in an action of contiguous particles, dependent on the forces developed in electrical excitement; these forces bring the particles into a state of tension or polarity, which constitutes both *induction* and *insulation*; and being in this state the contiguous particles have a power or capability of communicating these forces, one to the other, by which they are lowered and discharge occurs. Every body

a gold leaf electrometer and a charged carrier (an insulated metal ball of two or three inches diameter) upon it, the electrometer is diverged; removing the carrier, this divergence instantly falls, this is *insulation* and *induction*: if we replace the shell lac by metal, the carrier causes the leaves to diverge as before, but when removed, though after the shortest possible contact, the electroscope is left diverged, this is *conduction*. If we employ a plate of spermaceti instead of the metal, and repeat the experiment, we find the divergence partly falls and partly remains, because the spermaceti insulates and also conducts, doing both imperfectly: but the shell lac also conducts, as is shewn if time be allowed; and the metal also obstructs conduction, and therefore insulates, as is shewn by simple arrangements. For if a copper wire, 74 feet in length and $\frac{1}{12}$ of an inch in diameter, be insulated in the air, having its end *m* a metal ball; its end *e* connected with the earth, and the parts near *m* and *e* brought within half an inch of each other, as at *s*; then an ordinary Leyden jar being charged sufficiently, its outside connected with *e* and its inside with *m*, will give a charge to the wire, which instead of travelling wholly through it, though it be so excellent a conductor, will pass in large proportion through the air at *s*, as a bright spark: for with such a length of wire, the resistance in it is accumulated until it becomes as much, or perhaps even more, than that of the air, for electricity of such high intensity.



Admitting that such and similar experiments shew that conduction through a wire is preceded by the act of induction (1338), then all the phenomena presented by the submerged or subterranean wires are explained, and in their explanation confirm as I think, the principles given. After Mr. Wheatstone had, in 1834, measured the velocity of a wave of electricity through a copper wire, and given it as 288,000 miles in a second, I said, in 1838, upon the strength of these principles (1333,) "that the velocity of discharge through the *same wire* may be greatly varied, by attending to the circumstances which cause variations of discharge through spermaceti or sulphur. Thus, for instance, it must vary with the tension or intensity of the first urging force, which tension is charge and induction. So if the two ends of

appears to discharge (444.987); but the possession of this capability in a *greater or smaller degree* in different bodies, makes them better or worse conductors, worse or better insulators: and both *induction* and *conduction* appear to be the same in their principle and action (1320), except that in the latter, an effect common to both is raised to the highest degree, whereas in the former, it occurs in the best cases, in only an almost insensible quantity.

the wire, in Professor Wheatstone's experiment, were immediately connected with two large insulated metallic surfaces exposed to the air, so that the primary act of induction, after making the contact for discharge, might be in part removed from the internal portion of the wire at the first instant, and disposed for the moment on its surface jointly with the air and surrounding conductors, then I venture to anticipate, that the middle spark would be more retarded than before: and if these two plates were the inner and outer coating of a large jar, or a Leyden battery, then the retardation of that spark would be still greater." Now this is precisely the case of the submerged or subterraneous wires, except that instead of carrying their surfaces towards the inductive coatings (1483), the latter are brought near the former; in both cases the induction consequent upon charge, instead of being exerted almost entirely at the moment within the wire, is to a very large extent determined externally; and so the discharge or conduction being caused by a lower tension, therefore requires a longer time. Hence, the reason why, with 1500 miles of subterraneous wire, the wave was two seconds in passing from end to end; whilst with the same length of air wire, the time was almost inappreciable.

With these lights it is interesting to look at the measured velocities of electricity in wires of metal, as given by different experimenters.

	Miles per second.
* Wheatstone in 1834, with copper wire made it	288,000
* Walker in America, with telegraph iron wire	- 18,780
* O' Mitchell, ditto. - ditto. - -	- 28,524
* Fizeau and Gonnelle (copper wire)	- - 112,680
* Ditto. - - (iron wire) - - -	- 62,600
† A. B. G. (copper) London and Brussels Telegraph	2,700
† Ditto. (copper) London and Edinburgh Telegraph	7,600

Here, the difference in copper is seen by the first and sixth result to be above a hundred fold. It is further remarked in Liebig's report of Fizeau's and Gonnelle's experiments, that the velocity is not proportional to the conductive capacity, and is independent of the thickness of the wire. All these circumstances and incompatibilities appear rapidly to vanish, as we recognise and take into consideration the lateral induction of the wire carrying the current. If the velocity of a brief electric discharge is to be ascertained in a given length of wire, the simple circumstances of the latter being twined round a frame in small space, or spread through the air through a large space, or adhering to walls, or

* Liebig and Kopp's Report, 1850 (translated), p. 168.

† Athenæum, 14th January, 1854, p. 54.

lying on the ground, will make a difference in the results. And in regard to long circuits such as those described, their conducting power cannot be understood, whilst no reference is made to their lateral static induction, or to the conditions of intensity and quantity which then come into play; especially in the case of short or intermitting currents — for then static and dynamic are continually passing into each other.

It has already been said that the conducting power of the air and water wires are alike for a constant current. This is in perfect accordance with the principles and with the definite character of the electric force, whether in the static or current or transition state. When a voltaic current of a certain intensity is sent into a long water wire, connected at the further extremity with the earth, part of the force is in the first instance occupied in raising a lateral induction round the wire, ultimately equal in intensity at the near end to the intensity of the battery stream, and decreasing gradually to the earth end, where it becomes nothing. Whilst this induction is rising, that within the wire amongst its particles is beneath what it would otherwise be; but as soon as the first has attained its maximum state, then that in the wire becomes proportionate to the battery intensity, and therefore equals that in the air wire, in which the same state is (because of the absence of lateral induction) almost instantly attained. Then of course they discharge alike and therefore conduct alike.

A striking proof of the variation of the conduction of a wire by variation of its lateral static induction, is given in the experiment proposed 16 years ago (1333.) If, using a constant charged jar, the interval s , page 6, be adjusted so that the spark shall freely pass there (though it would not if a little wider), whilst the short connecting wires n and o are insulated in the air, the experiment may be repeated twenty times without a single failure; but if after that, n and o be connected with the inside and outside of an insulated Leyden jar, as described, the spark will never pass across s , but all the charge will go round the whole of the long wire. Why is this? The quantity of electricity is the same, the wire is the same, its resistance is the same, and that of the air remains unaltered; but because the intensity is lowered, through the lateral induction momentarily allowed, it is never enough to strike across the air at s ; and it is finally altogether occupied in the wire, which in a little longer time than before, effects the whole discharge. M. Fizeau has applied the same expedient to the primary voltaic currents of Ruhmkorff's beautiful inducing apparatus, with great advantage. He thereby reduces the intensity of these currents at the moment when it would be very disadvantageous, and gives us a striking instance of the advantage of viewing static and dynamic phenomena as the result of the same laws.

Mr. Clarke arranged a Bains' printing telegraph with three pens so that it gave beautiful illustrations and records of facts like those

stated : the pens are iron wires, under which a band of paper imbued with ferro-prussiate of potassa passes at a regular rate by clock-work ; and thus regular lines of prussian blue are produced whenever a current is transmitted, and the time of the current is recorded. In the case to be described, the three lines were side by side, and about 0.1 of an inch apart. The pen *m* belonged to a circuit of only a few feet of wire, and a separate battery ; it told whenever the contact key was put down by the finger ; the pen *n* was at the earth end of the long air wire, and the pen *o* at the earth end of the long subterraneous wire ; and by arrangement, the key could be made to throw the electricity of the chief battery into either of these wires, simultaneously with the passage of the short circuit current through pen *m*. When pens *m* and *n* were in action, the *m* record was a regular line of equal thickness, shewing by its length the actual time during which the electricity flowed into the wires ; and the *n* record was an equally regular line, parallel to, and of equal length with the former, but the least degree behind it ; thus indicating that the long air wire conveyed its electric current almost instantaneously to the further end. But when pens *m* and *o* were in action, the *o* line did not begin until some time after the *m* line, and it continued after the *m* line had ceased *i. e.* after the *o* battery was cut off. Furthermore, it was faint at first, grew up to a maximum of intensity, continued at that as long as battery contact was continued, and then gradually diminished to nothing. Thus the record *o* shewed that the wave of power took time in the water wire to reach the further extremity ; by its first faintness, it shewed that power was consumed in the exertion of lateral static induction along the wire ; by the attainment of a maximum and the after equality, it shewed when this induction had become proportionate to the intensity of the battery current ; by its beginning to diminish, it shewed when the battery current was cut off ; and its prolongation and gradual diminution shewed the time of the outflow of the static electricity laid up in the wire, and the consequent regular falling of the induction which had been as regularly raised.

With the pens *m* and *o* the conversion of an intermitting into a continuous current could be beautifully shewn ; the earth wire by the static induction which it permitted, acting in a manner analogous to the fly wheel of a steam engine, or the air spring of a pump. Thus when the contact key was regularly but rapidly depressed and raised, the pen *m* made a series of short lines separated by intervals of equal length. After four or more of these had passed, then pen *o*, belonging to the subterraneous wire, began to make its mark, weak at first, then rising to a maximum, but always continuous. If the action of the contact key was less rapid, then alternate thickening, and attenuations appeared in the *o* record ; and if the introductions of the electric current at the one end of the earth wire were at still longer intervals, the records of action at the other end became entirely separated from each other. All shewing most beautifully, how the individual current or wave, once introduced into the wire, and

never ceasing to go onward in its course, could be affected in its intensity, its time, and other circumstances, by its partial occupation in static induction.

By other arrangements of the pens *n* and *o*, the near end of the subterraneous wire could be connected with the earth immediately after separation from the battery; and then the back flow of the electricity, and the time and manner thereof, were beautifully recorded; but I must refrain from detailing results which have already been described in principle.

Many variations of these experiments have been made and may be devised. Thus the ends of the insulated battery have been attached to the ends of the long subterraneous wire, and then the two halves of the wire have given back opposite return currents when connected with the earth. In such a case the wire is positive and negative at the two extremities, being permanently sustained by its length and the battery, in the same condition which is given to the short wire for a moment by the Leyden discharge (p. 351); or, for an extreme but like case, to a filament of shell lac having its extremities charged positive and negative. Colomb pointed out the difference of long and short as to the insulating or conducting power of such filaments, and like difference occurs with long and short metal wires.

The character of the phenomena described in this report, induces me to refer to the terms *intensity* and *quantity* as applied to electricity; terms which I have had such frequent occasion to employ. These terms, or equivalents for them, cannot be dispensed with by those who study both the static and the dynamic relations of electricity; every current where there is resistance has the static element and induction involved in it, whilst every case of insulation has more or less of the dynamic element and conduction; and we have seen that with the same voltaic source, the same current in the same length of the same wire, gives a different result as the intensity is made to vary, with variations of the induction around the wire. The idea of intensity or the power of overcoming resistance, is as necessary to that of electricity, either static or current, as the idea of pressure is to steam in a boiler, or to air passing through apertures or tubes: and being independent of the idea of quantity, we must have language competent to express both relations. Furthermore, I have never found either of these terms lead to any mistakes regarding electrical action, or give rise to any false view of the character of electricity or its unity. I cannot find other terms of equally useful significance with these; or any which, conveying the same ideas, are not liable to such misuse as these may be subject to. It would be affectation, therefore, in me, to search about for other words; and besides that, the present subject has shewn me more than ever their great value and peculiar advantage in electrical language.

[M. FARADAY.]

The fuze referred to in page 97, is of the following nature. Some copper wire was covered with sulphuretted gutta percha ; after some months it was found that a film of sulphuret of copper was formed between the metal and the envelope ; and further, that when half the gutta percha was cut away in any place, and then the copper wire removed for about $\frac{1}{4}$ of an inch, so as to remain connected only by the film of sulphuret adhering to the remaining gutta percha, an intensity battery could cause this sulphuret to enter into vivid ignition, and fire gunpowder with the utmost ease. The experiment was shewn in the Lecture-room, of firing gunpowder at the end of eight miles of single wire. Mr. Faraday reported that he had seen it fired through 100 miles of covered wire immersed in the canal, by the use of this fuze.

Friday, January 27.

COL. PHILIP J. YORKE, F.R.S., Pres. Chem. Soc., in the Chair.

JOHN TYNDALL, Esq., Ph. D., F.R.S.,

PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION.

On the Vibration and Tones produced by the Contact of Bodies having different Temperatures.

IN the year 1805, M. Schwartz, an inspector of one of the smelting works of Saxony, placed a cup-shaped mass of hot silver upon a cold anvil, and was surprised to find that musical tones proceeded from the mass. In the autumn of the same year, Professor Gilbert of Berlin visited the smelting works and repeated the experiment. He observed, that the sounds were accompanied by a quivering of the hot silver, and that when the vibrations ceased, the sound ceased also. Professor Gilbert merely stated the facts, and made no attempt to explain them.

In the year 1829, Mr. Arthur Trevelyan, being engaged in spreading pitch with a hot plastering iron, and once observing that the iron was too hot for his purpose, he laid it slantingly against a block of lead which chanced to be at hand; a shrill note, which he compared to that of the chanter of the small Northumberland pipes, proceeded from the mass, and, on nearer inspection, he observed that the heated iron was in a state of vibration. He was induced by Dr. Reid of Edinburgh to pursue the subject, and the results of his numerous experiments were subsequently printed in the Transactions of the Royal Society of Edinburgh.

On the 1st of April, 1831, these singular sounds and vibrations formed the subject of a Friday Evening Lecture by Professor Faraday, at the Royal Institution. Professor Faraday expanded and further established the explanation of the sounds given by Mr. Trevelyan and Sir John Leslie. He referred them to the tapping of the hot mass against the cold one underneath it, the taps being in many cases sufficiently quick to produce a high musical note. The alternate expansion and contraction of the cold mass at the points where the hot rocker descends upon it, he regarded as the sustaining power of the vibrations. The superiority of lead he ascribed to its great expansibility, combined with its feeble power of conduction, which latter prevented the heat from being quickly diffused through the mass.

Professor J. D. Forbes of Edinburgh was present at this Lecture,

and not feeling satisfied with the explanation, undertook the further examination of the subject; his results are described in a highly ingenious paper communicated to the Royal Society of Edinburgh in 1833. He rejects the explanation supported by Professor Faraday, and refers the vibrations to "a new species of mechanical agency in heat"—a repulsion exercised by the heat itself on passing from a good conductor to a bad one. This conclusion is based upon a number of general laws established by Professor Forbes. If these laws be correct, then indeed a great step has been taken towards a knowledge of the intimate nature of heat itself, and this consideration was the Lecturer's principal stimulus in resuming the examination of the subject.

He had already made some experiments, ignorant that the subject had been further treated by Seebeck, until informed of the fact by Professor Magnus of Berlin. On reading Seebeck's interesting paper, he found that many of the results which it was his intention to seek had been already obtained. The portion of the subject which remained untouched was, however, of sufficient interest to induce him to prosecute his original intention.

The general laws of Professor Forbes were submitted in succession to an experimental examination. The first of these laws affirms that "*the vibrations never take place between substances of the same nature.*" This the Lecturer found to be generally the case when the hot rocker rested upon a *block*, or on the edge of a thick plate of the same metal; but the case was quite altered when a thin plate of metal was used. Thus, a copper rocker laid upon the edge of a penny-piece did not vibrate permanently; but when the coin was beaten out by a hammer, so as to present a thin sharp edge, constant vibrations were obtained. A silver rocker resting on the edge of a half-crown refused to vibrate permanently; but on the edge of a sixpence continuous vibrations were obtained. An iron rocker on the edge of a dinner knife gave continuous vibrations. A flat brass rocker placed upon the points of two common brass pins, and having its handle suitably supported, gave distinct vibrations. In these experiments the plates and pins were fixed in a vice, and it was found that the thinner the plate, within its limits of rigidity, the more certain and striking was the effect. Vibrations were thus obtained with iron on iron, copper on copper, brass on brass, zinc on zinc, silver on silver, tin on tin. The list might be extended, but the cases cited are sufficient to shew that the proposition above cited cannot be regarded as expressing a "general law."

The second general law enunciated by Professor Forbes is, that "*both substances must be metallic.*" This is the law which first attracted the Lecturer's attention. During the progress of a kindred enquiry, he had discovered that certain non-metallic bodies are endowed with powers of conduction far higher than has been hitherto supposed, and the thought occurred to him that such bodies might, by suitable treatment, be made to supply the place of metals in the production of vibrations. This anticipation was realized. Rockers of silver,

copper, and brass, placed upon the natural edge of a prism of rock-crystal, gave distinct tones; on the clean edge of a cube of fluor spar, the tones were still more musical; on a mass of rock-salt the vibrations were very forcible. There is scarcely a substance, metallic or non-metallic, on which vibrations can be obtained with greater ease and certainty than on rock-salt. In most cases a high temperature is necessary to the production of the tones, but in the case of rock-salt the temperature need not exceed that of the blood. A new and singular property is thus found to belong to this already remarkable substance. It is needless to enter into a full statement regarding the various minerals submitted to experiment. Upwards of twenty non-metallic substances had been examined by the Lecturer, and distinct vibrations obtained with every one of them.

The number of exceptions here exhibited far exceeds that of the substances which are mentioned in the paper of Professor Forbes, and is, it was imagined, sufficient to shew that the second general law is untenable.

The third general law states, that "The vibrations take place with an intensity proportional (within certain limits) to the difference of the conducting powers of the metals for heat, the metal having the least conducting power, being necessarily the coldest." The evidence adduced against the first law appears to destroy this one also; for if the intensity of the vibrations be proportional to the difference of the conducting powers, then, where there is no such difference, there ought to be no vibrations. But it has been proved in half a dozen cases, that vibrations occur between different pieces of the same metal. The condition stated by Professor Forbes was, however, reversed. Silver stands at the head of conductors; a strip of the metal was fixed in a vice, and hot rockers of brass, copper, and iron, were successively laid upon its edge: distinct vibrations were obtained with all of them. Vibrations were also obtained with a brass rocker which rested on the edge of a half-sovereign. These and other experiments shew that it is not necessary that the worst conductor should be the cold metal, as affirmed in the third general law above quoted. Among the metals, antimony and bismuth were found perfectly inert by Professor Forbes; the Lecturer however had obtained musical tones from both of these substances.

The superiority of lead as a cold block, Professor Faraday, as already stated, referred to its high expansibility, combined with its deficient conducting power. Against this notion, which he considers to be "an obvious oversight," Professor Forbes contends in an ingenious and apparently unanswerable manner. The vibrations, he urges, depend upon the difference of temperature existing between the rocker and the block; if the latter be a bad conductor and retain the heat at its surface, the tendency is to bring both the surfaces in contact to the same temperature, and thus to stop the vibration instead of exalting it. Further, the greater the quantity of heat transmitted from the rocker to the block during contact, the greater must be the expansion, and hence, if the vibrations be due to this

cause, the effect must be a maximum when the block is the best conductor possible. But Professor Forbes, in this argument, seems to have used the term expansion in two different senses. The expansion which produces the vibration is the sudden upheaval of the point where the hot rocker comes in contact with the cold mass underneath; but the expansion due to good conduction would be an expansion of the general mass. Imagine the conductive power of the block to be infinite, that is to say, that the heat imparted by the rocker is instantly diffused equally throughout the block; then, though the general expansion might be very great, the local expansion at the point of contact would be wanting, and no vibrations would be possible. The inevitable consequence of good conduction is, to cause a sudden abstraction of the heat from the point of contact of the rocker with the substance underneath, and this the Lecturer conceived to be the precise reason why Professor Forbes had failed to obtain vibrations when the cold metal was a good conductor. He made use of *blocks*, and the abstraction of heat from the place of contact by the circumjacent mass of metal, was so sudden as to extinguish the local elevation on which the vibrations depend. In the experiments described by the Lecturer, this abstraction was to a great extent avoided, by reducing the metallic masses to thin laminæ, and thus the very experiments adduced by Professor Forbes against the theory supported by Professor Faraday, appear, when duly considered, to be converted into strong corroborative proofs of the correctness of the views of the philosopher last mentioned.

[J. T.]

Friday, February 3.

Right Hon. BARON PARKE, Vice-President, in the Chair.

W. R. GROVE, Esq., Q.C., F.R.S.

On the Transmission of Electricity by Flame and Gases.

IN the year 1730, Mr. Stephen Grey, a pensioner of the Charter House, was led by pursuing a series of ingenious experiments, to the important discovery that bodies might be divided into two classes, conductors and non-conductors of Electricity. Subsequent discoveries led to the knowledge that different bodies conduct Electricity, not only very differently as to degree, but also differently as to mode, and as to the changes which the bodies themselves experience while conducting electricity. We thence get conduction without apparent change, as by the metals,—conduction dependent upon chemical change, as by Electrolytes; and then again in effects of transmission not usually included under the term conduction, we get the discharge by carrying or convection, and by disruption, as in the spark.

In the year 1852, Mr. Grove communicated at an Evening Meeting of the Members of the Royal Institution, his researches on the disruptive discharge, shewing by the oxidation and reduction of the terminal surfaces, a state of chemical polarity in the gaseous intervening medium, antecedent to the discharge, the discharge consisting of a subversion of this polarity attended with intense local heat, and a transmission of minute particles of the terminals between which it took place.*

In what may be termed Pneumatic Electricity, or the electrical effects produced on and by gases, there seems some reason to believe that the molecules conduct, or in other words that at indefinitely minute spaces, electricity can pass without the phenomena of disruptive discharge: for instance in the gas battery, either the molecules must conduct, or the gases must by contact with the platinum be brought into a liquid state.

The effects of rarefaction on gases (as by the air-pump) tends to render the disruptive discharge more facile, and to enable electricity of the same degree of intensity to pass across much larger spaces than it would when transmitted across gases in a dense state.

* Phil. Trans. 1852, p. 87.

The next enquiry is whether the effect of rarefaction by heat is the same as that by mechanical attenuation; and heated gas was shewn by Mr. Grove to facilitate the disruptive discharge of electricity: so strikingly was this evidenced with flame, that when the flame of a spirit lamp was held near the terminal point of the coil apparatus of Ruhmkorff, (the coatings of a Leyden phial being connected with the secondary coil, and the terminals being separated to a distance far beyond that at which the spark would pass in cold air), the spark darted to and along the margin of the flame, and could be curved or twisted about in any direction, at the will of the experimenter, giving a striking illustration of the crooked form of lightning, and of the probable reason why it does not pass in straight lines, the temperature of the air being different at different points of its passage, and much of this variation of temperature being in all probability occasioned by the mechanical effect on the air of the discharge itself.

No amount of rarefaction has hitherto shewn any thing like *conduction* in gases at ordinary temperatures; but on the other hand flame does give distinct evidence of conduction without disruptive discharge, and an experiment was made demonstrating this.

Is this effect of flame due simply to its consisting of highly heated gas? or is it due to the chemical action taking place throughout the whole structure of the flame?

When closely approximated metals are brought to the point of visible ignition, signs, but very feeble signs of transmission of electricity take place. M. E. Becquerel has recently published some very interesting experiments on this subject,* and Mr. Grove not by means of M. E. Becquerel's plan, but by igniting by the voltaic battery two platinum wires placed close to each other in an exhausted receiver, and connecting them with a third voltaic battery, had obtained slight deflections of a delicate galvanometer.

These effects are however far inferior to those shown by flame, and appear to depend more upon the state of the terminals than upon the state of the intervening gas:—thus, until the terminals attain a red heat, no transmission takes place, whatever be the degree of attenuation of the gas; while if the terminals have attained a red heat, the current is much more easily transmitted by rare than by dense gas. Thus alterations in the density of the gas do not appear to affect the transmission, until a certain definite change has taken place in the state of the terminals. Reasoning from these effects, and bearing in mind the effect of rarefied gas on the disruptive discharge and the quasi-radiation of matter in the phenomena elicited by Moser and others, Mr. Grove inclined to the opinion that the transmission across heated gas differed specifically from that across flame, the former being in some respects analogous to the disruptive, while the latter resembled the electrolytic discharge.

* Annales de Chimie, Nov. 1853.

Flame moreover has been observed to conduct better in one direction than another, and the question next arises will flame produce or generate a voltaic current? M. Hankel and M. Buff have published papers shewing, by the use of highly sensitive galvanometers, a current apparently produced by flame, which passes from the upper to the lower part of the flame. M. Buff attributes this current to thermo-electricity—the flame being a conductor and two metals in contact with different parts of it, the thermo-current passes from the hotter to the cooler metal, and hence the result.

Mr. Grove in studying this subject, and without having then read the papers of Hankel and Buff, found the results so varying in ordinary flame that he could come to no satisfactory conclusion; he was then led to think, that as in the flame of the blow-pipe, the direction or line of combustion is more definite than in ordinary flame, he might get more definite results. He experimented with the latter flame, and immediately got very distinct evidence of a current not due to thermo-electricity, as it could be made to conquer both the effect of the thermo-flame-current noticed by Buff, and of any thermo-current excited in the junction of the wires exterior to the flame.

This current which Mr. Grove termed the flame current proper, moves from the root towards the point of the blow-pipe flame—the best points for placing the collecting spirals or plates of platinum being for the one a little above the root or base of the blue cone, and for the other, in the full yellow flame a little beyond the apex of the blue cone.

As the latter metal is much more heated than the former, the thermo-flame-current is opposed to, and though it by no means destroys, it tends to weaken the effect of the flame current proper; if then this metal can be adventitiously cooled, we should have the two currents co-operating, instead of conflicting; and so experiment proved, for by using a capsule of platinum filled with water in the full flame, and a coil or sheet of platinum foil at the base, a very marked current resulted. By arranging in a row of jets worked by a large bellows a sheet of platinum foil placed just over the roots of the flames, and a trough of platinum foil filled with water just beyond the points of the blue cones, the large galvanometer of the Institution was deflected to 30° or 40° , so as to be easily visible to the audience; the deflection being in the reverse direction upon reversing the connections respectively with the plate and trough. The same apparatus will also readily decompose iodide of potassium; iodine being evolved at the platinum point in connection with the trough.

There was another apparatus on the table for arranging the flame battery as an intensity series. The direction of the current is from the points in the flame where combustion commences, to those where it concludes; it appears to be transmitted by a chain of chemical action taking place between these. Though speaking with some reserve on the theory of the phenomenon, Mr. Grove could at present see no objection to its being regarded as a current produced

by chemical action ; the platinum at the commencement of action representing the zinc which burns or combines with oxygen ; that at the conclusion, representing the platinum, or the points where chemical action concludes, and a tendency to reduction or de-oxidation is manifested. The distinction being that the generative chemical action, instead of taking place, as in the ordinary battery only at the zinc surface, and being simply transmitted by the electrolyte, takes place throughout the intervening section of flame ; and thus, within certain limits, the intensity of the electricity increases with the distance of the plates, instead of decreasing as in the ordinary battery.

[W. R. G.]

Friday, March 3.

GEORGE DODD, Esq., F.S.A., Vice-President, in the Chair.

REV. BADEN POWELL, M.A., V.P.R.S., F.R.A.S., F.G.S.,
SAVILIAN PROFESSOR OF GEOMETRY, OXFORD.

On certain Phenomena of Rotatory Motion.

THE mechanical principle of "the composition of Rotatory Motion," originally discovered by Frisi about 1750, (see Frisius de Rotatione, Op. ii. 134, 157, and Cosmographia, ii. 24) is equally simple in its nature, important and fertile in its consequences and applications,—and susceptible of the easiest explanation and experimental illustration; yet it has been singularly lost sight of in the common elementary treatises. It is indeed discussed and applied in a Mathematical form in Mr. Airy's Tract on Precession (Math. Tracts, p. 192, 2nd ed.); and the theorem is stated by Professor Playfair in his "Outlines of Natural Philosophy" (i. 144), and its application explained (ib. ii. 308). These, however, are not books of a popular kind, and the author is not aware of any mention of it in other English works. In a more abstract analytical form it has been discussed by several foreign mathematicians, especially by Poinsot, in a Memoir read to the Academy of Sciences, May 19, 1834, but of which only an abstract was published; as well as by Poisson, in a paper in the Journal de l'Ecole Polytechnique (xvi. 247).

The principle is involved in the explanation of several important phenomena, some of which are in fact mere direct instances of it; so that a simple experimental mode of exhibiting it would be eminently desirable; and several such have accordingly been devised which yet seem to have been but little generally known.

An ingenious instrument of the kind was contrived some years ago by Mr. H. Atkinson, a very brief account of which is given in the Astronomical Society's Notices, vol. i. p. 43, though so brief that it is difficult to collect what the precise mode of its action was,—but it seems somewhat complex.

A far more complete and instructive apparatus was invented by Bonenberger and described in Gilbert's Annalen (lx. p. 60). It is also explained in some German elementary works. Attention has been more recently drawn to the subject by a highly interesting paper of Professor Magnus of Berlin, (Abhandlungen der Königl. Preuss. Akad. 1852, translated in Taylor's Foreign Scientific Memoirs, N. S. Part 3, p. 210,) in which some remarkable applications of this apparatus are given; he also describes it (with a figure) and observes

that the execution of it requires great delicacy and correctness of workmanship. Copies of this instrument have indeed been made in this country (one of which was exhibited through the kindness of Professor Wheatstone); but of these the author believes no description has ever appeared in English works, and they are certainly very little known, notwithstanding their manifest value to every lecturer: the essential parts are a sphere capable of rotating about an axis whose extremities rest in opposite points of a hoop which can turn on pivots *horizontally*, within another hoop turning on pivots about a *vertical* line.

In fact the author of the present communication has long felt the want of such an apparatus for lecture illustration; and before he was aware of the existence of any of those just alluded to, had constructed one in a different form, and which is found to answer fully the purposes of illustration for which it is designed, without any nice workmanship or complex machinery. (See *Astronomical Society's Notices*, vol. xiii. p. 221-248.)

Its object, like that of the instrument last mentioned, is to exhibit experimentally *the actual composition of rotations about two different axes impressed at once on the same body*.

The essential parts are merely a bar capable of rotating freely about one end of an axis, (and loaded at its extremities to keep up the rotation,) while the axis itself can turn about a point in its length near the end carrying the bar, upon a horizontal axis, capable of moving freely round a vertical pillar. At the lower end of the first axis is a weight which more than counterpoises the upper part.

If then there be no rotation in the bar about the first axis, the effect of the weight is to produce a rotation about the second alone, bringing down the first axis into a vertical position.

If now the first axis be held horizontally or obliquely, and a rotatory motion be given to the bar about it, on letting the axis go, *we compound both rotations*; and the resulting effect is, that the weight will no longer bring the axis *down*, or alter its *inclination* at all: but will cause it to take a new position, or make the whole to turn round the vertical, in a direction *opposite* to that of the rotation.

Thus, although confessedly not new in principle, to make public an experimental illustration in so simple a form may not be without its use for a great majority of students.

Even the theoretical principle is capable of being stated in a way quite intelligible to those acquainted only with the very first rudiments of theoretical mechanics, presenting itself in close *analogy* to that well known first principle, the composition of *rectilinear* motion.

As in this last case, if a body be in motion in one direction, and any cause tend to make it move in another, it will move in neither, but in an intermediate direction,—so we have the strictly analogous case in *rotatory* motion; *when a body is rotating about an axis, and*

any cause tends to make it rotate about another axis, it will not rotate about either, but about a new axis intermediate to the two. Thus the result of compounding the two rotations will be, that the axis (carrying with it the rotating body) will simply take a new position, or will move in a direction determined by the nature of the impressed motions.

Professor Magnus, in the very able, but rather prolix and obscurely written Memoir, before referred to, speaks (p. 223) of the consequences of such a law as evinced in the resulting rotations, but without any distinct or explicit statement of the essential theorem of the composition of rotatory motion. He gives, however, some singular and even paradoxical exemplifications of it. We may allude to one of these, which is capable of being put into a form at once *more simple*, and at the same time *more paradoxical*, than that in which he describes it. It consists in this: a wheel at one end of an axis, and a weight at the other, are suspended in equilibrio; which is, of course, unaltered, whether the wheel be at rest or in rotation: the weight is then slid so that the balance is *destroyed*: now if the wheel be set in rapid *rotation*, the *equilibrium is restored*. This is nothing but a simple case of the principle just stated, as shewn by the author's apparatus.

Besides certain other cases traceable to a different cause, Professor Magnus's immediate object is to explain a curious observed anomaly in the motion of projectiles of an *elongated* form shot from *rifled* guns, and which consequently *rotate* about their axis, while passing through the air in the direction of that axis.

He mentions the fact that artillery experiments in different countries with rifled cannon and missiles of a cylindrical form with a conical apex, *always shew a deviation of the point of the missile to the right, the rifle-spiral being right-handed*.

To explain the nature of this deviation was the object of special experiments on the part of the Prussian Artillery Commission, in which Professor Magnus assisted. The missiles were fired with low charges, so as to allow the motion to be accurately observed, and it was found that the axis remained sensibly in the direction of the tangent to the curved path, while the deviation to the right was always clearly marked. He observes that left-handed rifles have never been tried.

Professor Magnus, after some fruitless conjectures as to the cause, at length sought it in the principle of *the composition of rotatory motion*. He tried experimentally the effect of a current of air on a projectile of the form employed, by inserting such a body instead of the rotating sphere in Bonenberger's apparatus, and observing the effect on it, first at rest, and then in rotation, when the strong current of a blowing machine was directed against the conical apex. When at *rest*, the current *elevated* the apex; owing to the form of the missile the resistance acting not through the centre of gravity, but above it: when in *rotation* no *elevation* took place, but a *deviation*

in the direction of the axis, in a direction opposite to that of rotation. To shew the application of the principle in this case, he observes that the axis of the elongated projectile, which for an instant coincides with the *tangent* to its curved path, momentarily changes its direction, so that the front extremity or apex falls below its former position. Or, for a single instant it may be regarded as if locally at rest, but turning about its centre of gravity so as to depress the apex.

If the motion were simply in the direction of the axis, the *resistance of the air* would operate directly against it; but when the apex is continually tending to turn *downwards* from that line, the resistance acts against it partially *upwards*, and thus tends to raise the apex.

Thus, at a given instant, the elongated projectile may be represented by the rotating part of the apparatus just described.

When there is *no rotation*, the resistance of the air tending to raise the apex is represented by the weight at the lower end, which produces the same effect.

When a rapid *rotation* is communicated, (suppose from left to right of the gunner,) the result will be, *no elevation of the apex*, but a *lateral* movement, or commencement of a rotation round the vertical,—in astronomical language *retrograde*, if the former rotation be *direct*;—but which beginning from the opposite part of the circle is, *relative to the operator*, towards the *right*.

The form of the projectile used in these experiments differs from that in the Minie rifle, in that the latter is hollow at its broader end, and thus the centre of gravity is thrown forward towards the apex. Hence, according to the same theory, the effect would probably here be to depress the apex, and therefore to give an opposite deviation: but it does not appear whether any such observations have been made; and in practice the effect would probably be quite insensible.

It occurred to the author that a very simple illustration of this deviation of rifle projectiles might be made by merely forming a sort of small arrow, whose head was composed of a cork, like a shuttlecock, but instead of the feathers, small card vanes inclined in the same direction round it, with a tail to balance it, and which thus in the mere act of throwing acquires a rotatory motion from the reaction of the air, to the right or left according as the vanes are inclined; and on trying this there was always observed a deviation in the direction of the axis or point of the missile to the right or left accordingly, relative to the experimenter. It is in fact nearly impossible to throw such a body in a direction perfectly in one plane. The true deviation is, however, peculiarly liable to be disguised by the general resistance of the air on so light a missile, as well as by currents, &c. which it is not easy to guard against.

The well known case of the *Boomerang* exhibits effects closely similar: for it is found that if so projected that its *rotation* is from

left to right, its *deviation* will be in the *same direction*, and *vice versé*: that is, supposing (as is the usual case) that its plane is inclined *upwards* from the operator:—If it be inclined *downwards*, the deviation is in the direction *opposite* to that of the rotation.

In the former case the reaction of the air against the flat surface of the missile would tend to increase its inclination *upwards*, in the latter *downwards*, with respect to the operator: and this in each case respectively would give the motion stated; as is easily seen on the principle, and by means of the apparatus, before described.

Thus it would follow that this extraordinary instance of savage invention, which long ago puzzled inquirers, is simply a case (like the last) of “the composition of rotatory motion.”

It should, however, be mentioned that some experimentalists have entertained a different view of the cause of deviation in this instance.

Besides the results above stated, Professor Magnus (in the same Memoir) mentions several other highly curious cases produced by certain modifications of the apparatus; but all referrible to the same principles.

M. Fessel has also invented an apparatus (since called the Gyroscope) an account of which is given with some remarks by Professor Plücker, and the Editor in Poggendorff's *Annalen* (1853, Nos. 9 and 10), which though apparently invented without any knowledge of Bonenberger's apparatus, is a modification of it, referring to phenomena of the same kind as those of the equilibrium experiment mentioned at first.

This apparatus has been greatly improved upon by Professor Wheatstone, who has introduced other movements to include the conditions of rotation in different planes. One of these instruments was exhibited.

From these singular applications of a very simple mechanical truth, we may now turn to what is but another exemplification of the same thing, however apparently remote from those we have considered, and upon a far grander scale.

The phenomenon of the Precession of Equinoxes was known to Hipparchus; but no explanation of the fact was for ages imagined. Even Kepler, in the multiplicity of his hypothetical resources, could not succeed in devising anything plausible. The axis of the Earth is slowly shifting its position, so that its pole points continually to a new part of the heavens,—a new pole star,—at the rate of about 50" a year, and of course carries with it the point of intersection of the Earth's equator with the ecliptic or plane of its orbit, at the same rate and in a direction opposite to that of its motion, or the order of the signs.

These phenomena remained wholly without explanation till Newton, led by the analogy of those disturbing forces on the orbit of a planet which cause its *nodes* to *regress*, shewed that the same would occur in a *satellite* to the earth,—in a *ring* of such satellites,—in such a ring adhering to the equator, or the protuberant part

of the terrestrial sphere; and thus that the equinoctial points would slowly regress. (See *Principia*, i. 66, Corr. 11—22.)

The more exact determination of quantitative results was reserved for Newton's successors, when a more powerful analysis had been applied by Euler, D'Alembert and others to the full exposition of the theory, founded on general equations of motion; as since given in the writings of Laplace, (*Mec. Cel.* liv. xiv. ch. 1.) and Pontécoulant (*Théorie du Système du Monde*, liv. iv. ch. 5.), which are necessary for including all the minuter variations detected by Bradley, and subsequent observers, shewing the *nutation* of the axis, and the inequalities of precession due to the varying configurations of the attracting luminaries.

These higher mathematical views, though of course the most complete and systematic, are not the most direct or easy mode of explaining the subject to the student. Greater simplicity certainly characterizes the method adopted by Mr. Airy (in the tract before cited) of applying *directly* the theorem of the composition of rotatory motion; as doubtless Newton would have done had it been known to him. But here, as in so many other instances, the first explanation presented itself mixed up with more complex considerations; and as has been well observed "simplicity is not always a fruit of the first growth."

To those not versed in the mathematical theory, of all points in Physical Astronomy, the "modus operandi" of the Precession, perhaps, usually seems the most paradoxical, and the explanations given in some of the best popular treatises are seldom found satisfactory, following as they do the letter of Newton's illustration and omitting the *direct* introduction of the principle of composition, which, if only from what has been here offered, is at once seen to be easily capable of the most elementary explanation. Indeed it was from this consideration forcing itself on the mind of the author, in several courses of popular lectures on Astronomy, that he was led to seek the means of experimental illustration above described; and which would more palpably imitate the phenomena to the eye, if, instead of the rotating *bar* a terrestrial *globe* be substituted (as in Bonenberger's instrument)—for better illustration made protuberant at the equator,—where the weight at the south pole acts the part of the sun's and moon's attraction, to pull down the protuberant matter of the spheroid at the equator if at *rest*, but when combined with the earth's *rotation* results in a transference of the position of its axis, or slow revolution of its pole round the pole of the ecliptic in a direction opposite to its rotation, carrying with it the equinoctial points, and causing the *signs* of the zodiac to shift backwards from their respective *constellations*.

It always affords a sort of intellectual surprise, to perceive for the first time the application of some simple and familiar mechanical principle to the grand phenomena of astronomy: to see that it is but one and the same set of laws which governs the motions of

matter on the earth and in the most distant regions of the heavens; to find the revolution of the apsides in a pendulum vibrating in ellipses, or the conservation of areas in a ball whirled round by a string suddenly shortened: or (as in the present case) to perceive a celestial phenomenon, vast in its relations both to time and space and complex in its conditions, identified, as to its mechanical cause, with the rotatory movement of a little apparatus on the table before us,—or to discover the Precession of Equinoxes in the deviation of a rifle or a boomerang. And the simple experimental elucidation of such phenomena and their laws will not be useless, as it tends to confirm in the mind of the student the great characteristic of the modern physical philosophy first asserted by Galileo, the identity of the causes of the celestial and terrestrial motions, and to aid and elevate our conception of those grand and simple principles according to which the whole machinery of the universe is so profoundly adjusted.

[B. P.]

Friday, March 10.

Right Hon. BARON PARKE, Vice-President, in the Chair.

CHARLES BROOKE, M.A., F.R.S.

SURGEON TO THE WESTMINSTER HOSPITAL.

On the Construction of the Compound Achromatic Microscope.

MR. BROOKE stated his motive in giving the Lecture to be an observation frequently made, that many who are in possession of the best microscopes, either for the purpose of pursuing original investigations, or of seeking rational recreation in acquiring a knowledge of the structure of natural objects, do not develop the full power of their instruments, from a want of sufficient acquaintance with the principles on which the definition of objects depends.

After briefly adverting to the ordinary phenomena of reflection, the Lecturer illustrated those of refraction by a movable diagram, which readily explained the total reflection of a ray of light incident on the common surface of two media at an angle greater than the critical angle, corresponding to which the angle of refraction is 90° .

The aberration of rays reflected or refracted at a spherical surface was then alluded to; and although the reflectors employed in microscopes may be rendered free from spherical aberration by giving them an elliptic, and those of telescopes, a parabolic form, there is no practicable method at present known of constructing lenses otherwise than with spherical or plane surfaces: and from the difficulty of obtaining sufficiently perfect reflecting surfaces, and of preserving them when obtained, refracting microscopes are now almost universally employed.

Chromatic dispersion was then mentioned, and the usual mode of producing achromatism by the combination of various kinds of glass, which differ in their dispersive power, was illustrated by a combination of three prisms. The construction of achromatic object-glasses was next explained, as well as the nature of the aberration produced by the presence or absence of a plate of thin glass covering the object, and the mode of correcting it in object-glasses of high power, by varying the distance of the anterior from the posterior combinations, as first extensively applied in practice by Mr. A. Ross, and fully detailed in his article on the Microscope, in the Penny Cyclopædia.

The angle of aperture of object-glasses was then explained, and the power of those of large angular aperture in developing the

structure of certain test objects, such as the siliceous shells of diatomaceæ, was explained to be totally distinct from the mere increase of the amount of light transmitted. Mr. Brooke offered an hypothesis as to the structure of these objects, from which it would follow that that structure would be rendered visible by oblique rays *alone*, and the necessary degree of obliquity would depend on the smallness of the elevations on the undulating surface of the shell. This view was thus shewn to be highly probable; a specimen of the *Pleurosigma formosum* (first found by Mr. Brooke at Walton-on-the-Naze) was viewed under a half inch object-glass by Ross, and an achromatic eye-piece of high power, (which was stated to be unquestionably superior to a deep Huygenian eye-piece); when an opaque disc was interposed between the object and the centre of the object-glass, which cut off a large portion of the central rays, the diagonal rows of dots were still distinctly visible; but when the marginal rays were stopped out by a diaphragm, although a much larger quantity of light was admitted than in the former case, the markings were entirely lost.

In order to render visible the more difficult objects of this class, glasses of large angle of aperture have been constructed, but their employment is much limited, owing to the greatly increased difficulty of correcting the aberrations, under any given circumstances of the transmitted pencil of light, and consequently the small amount of correction, that is, of adaptation to altered circumstances, that they admit of. From investigations which he knew to be in progress, the Lecturer expressed a hope that by due adjustments of the illuminating pencil, the most difficult test-objects would be rendered equally visible under object-glasses of moderate aperture, which are much more generally useful.

Mr. Brooke then alluded to the preposterous reputed angle of aperture of certain foreign object-glasses, viz. 172° , and explained the fallacy of the ordinary method of determining that angle; which consists in viewing through a microscope the light of a lamp placed at a few feet distance, and moving either the light or the microscope, so as to traverse the entire angular distance through which the light is visible. In this method the course of the rays is contrary to their usual course, and oblique pencils may be brought to an imperfect focus at the back of the object-glass, and produce a glare of light, but which meet at a greater angle than the extreme rays that can enter the object glass *from the field of view*, and which consequently are the extreme available rays.

A very perfect instrument for measuring the angle of aperture, designed by Mr. Gillett, was then explained: this consists of two microscopes, the optical axes of which may be adjusted to coincidence. One of these is attached horizontally to the traversing

arm of a horizontal graduated circle, and is adjusted so that the point of a needle, made to coincide with the axis of motion of the movable arm, may be in focus and in the centre of the field of view. The other microscope, to which the object-glass to be examined is attached, is fixed, and so adjusted, that the point of the same needle may be in focus in the centre of its field. The eye-piece of the latter is then removed, and a cap with a very small aperture is substituted, close to which a lamp is placed. It is evident that the rays transmitted by the aperture will pursue *the same course* in reaching the point of the needle, as the visual rays from that point to the eye, but in a *contrary direction*, and being transmitted through the movable microscope, the eye will perceive an image of the bright spot of light throughout that angular space that represents the true aperture of the object glass examined. The applications of this instrument in the construction of object-glasses are too numerous to be here detailed : amongst the most obvious of which may be mentioned the ready means it presents of determining the nature, and measuring the amount of the aberration in any given optical combination.

The important subject of Illumination was then so far considered as the short space of time allotted to the discourse would permit. It may be taken as an axiom that in the illumination of transparent objects, the amount of definition will depend on the accuracy with which the illuminating rays converge upon the several points of an object ; consequently the source of light and the field of view must be the *conjugate foci* of the illuminator, of which an achromatic combination, similar to an object-glass, is the best form, and the common mirror usually employed is probably the worst, inasmuch as in a pencil of rays obliquely reflected at a spherical surface, no focal point exists.

The first compound microscopes on record, as those of P. Bonnani, about 1697, which was placed horizontally, and that of J. Marshall in the beginning of the eighteenth century, which was vertical, were furnished with central condensers, but in later years the perfection of the illuminating apparatus has by no means kept pace with that of the ocular portion of the microscope, though scarcely of less importance, in attaining the utmost practicable perfection in the vision of microscopic objects.

The advantages of employing an achromatic condenser were first pointed out by Dujardin, since which time an object-glass has been frequently, but inconveniently employed, and more recently achromatic illuminators have been constructed by most of our instrument-makers.

Some years since Mr. Gillett was led by observation to appreciate the importance of controlling not merely the *quantity of light*, which may be effected by a diaphragm placed any where between the source of light and the object, but the *angle of aperture of the*

illuminating pencil, which can be effected only by a diaphragm placed immediately behind the achromatic illuminating combination. An elastic diaphragm, or *artificial pupil* as it might be called, was first proposed by Mr. Brooke, which was shewn to answer very well in a large model, and produced a remarkable semblance of vital contractility; but mechanical difficulties interfered with its application, and the revolving diaphragm in the instrument, now well known as Gillett's condenser, was substituted.*

When the rays of light converging on the field of view meet at a greater angle than that of the extreme rays that can enter the object-glass, the dark-ground illumination is produced, in which the objects are seen in strong lines of light on a dark ground; this is best suited to objects having a well-marked outline, such as the spicula of sponge, or the shells of the polygastrica. This may be effected either by Wenham's truncated parabolic reflector, or by a central opaque stop in Gillett's condenser.

The value of this kind of illumination in certain cases was shewn by its effect in rendering visible the persistent cell-walls in a specimen of hard vegetable tissue, a section of a plum-stone, which could hardly be distinguished by the ordinary, or bright-ground illumination.

A white cloud brightly illuminated by the sun has long been recognised as the best source of illumination, but as this is not often obtainable, the light of a lamp thrown upon a flat surface of plaster of Paris, or powdered carbonate of soda, has been used as a substitute. A flat surface of white enamel finely ground, but not polished, has been used with advantage by Mr. Gillett, as the surface can always be rendered perfectly clear by a little soap and water. By either of these means the glare resulting from throwing the unmodified light of a lamp on the object is completely obviated.

The effect of glare or diffused light in interfering with the vision of an object was illustrated by reference to an experiment of Professor Faraday's, in which a screen of gauze partially blackened is held in front of a printed placard or diagram; the diffused light reflected from the white gauze considerably obscures the object, which is scarcely interfered with by the blackened portion.

The influence of illumination upon definition was rendered very evident by placing the two halves of a fly's tongue, similarly mounted, under two microscopes having precisely similar object-glasses and eye-pieces; the one was carefully illuminated by an achromatic condenser, and artificial white cloud; the other, by the light of a similar lamp reflected from a concave mirror: the dif-

* A description of this very useful apparatus has been recently published in the "Elements of Natural Philosophy," by Golding Bird and Charles Brooke.

ference was so conspicuous, that some were inclined to doubt the identity of the objects.

The whole subject of the illumination of opaque objects, as well as that of oblique illumination, by Kingsley's condenser, and by the prisms of Nachet and Amici, of which diagrams were exhibited, and by other means, was unavoidably omitted

Microscopes by the three leading makers were placed on the table ; between the optical parts of which Mr. Brooke declined the task of drawing any invidious distinctions. He however expressed a preference for the stand of Mr. Ross, on account of its having a secondary stage with rectangular adjustments, and a rotatory movement by which any illuminating apparatus may be made to revolve after its axis has been brought to coincide with that of the microscope.

A stand by Mr. Ladd was also exhibited, in which the various movements are effected with great smoothness, and without "loss of time," by means of wrapping chains : also the ingenious apparatus of Mr. Highley, for obtaining photographs of microscopic objects, of which time did not admit of any explanation being offered.

In a curious and complicated microscope, the property of Prof. Quekett, constructed about the middle of the last century by Benjamin Martin, might be noticed several points of construction, that have been introduced as recent improvements.

[C. B.]

Friday, March 31.

COL. PHILIP J. YORKE, F.R.S. President of the Chemical Society,
in the Chair.

JOHN HALL GLADSTONE, Esq., Ph. D., F.R.S.

On Chemical Affinity among Substances in Solution.

AN historical sketch of the development of the ideas of chemists concerning "Affinity" was first given. The dogma of Hippocrates that "Like combines only with like," was shewn to be superseded by the view of Glauber and others, that unlike substances combine most readily: and that where two bodies have an affinity *for* one another, it is a sign that they have *no* affinity *with* one another. The views of Newton and Boyle in reference to the different degrees of strength of affinity were then considered, and particular attention was directed to the doctrine of Bergmann, that when a decomposition takes place by means of the greater elective attraction of a third body, that decomposition is complete. In opposition to this, Berthollet contended that in all such cases of composition, or decomposition, there takes place a partition of the base, or subject of the combination, between the two bodies whose actions are opposed; and that the proportions of this partition are determined, not solely by the difference of energy in the affinities, but also by the difference of the quantities of the bodies,—by their physical condition,—and by that of the combinations capable of being generated. These views did not meet with a favourable reception at the time of their promulgation; and the attention of chemists had been drawn away from the subject until within these last few years, when Malaguti, Bunsen, Debus, and Williamson, have published investigations bearing upon the point. The Lecturer then stated that before any of these papers had appeared, he had been thinking of and performing some experiments upon the subject in question, and that he was still continuing them.

After a few experiments illustrative of "Chemical combination" and of "Elective Affinity," others were introduced to show how easily this latter phenomenon was affected by circumstances. Thus ammonia will displace alumina from a solution of the sulphate, but on the other hand, alumina will displace ammonia when heated with the solid sulphate of that volatile base; whilst if solutions of chloride of aluminum and sulphate of ammonia be mixed and evaporated, crystals of the double sulphate, ammonia-alum, will appear. There were on the table two white salts; the one had been carbonate of

baryta, but by boiling with excess of sulphate of potash, it had been converted into the sulphate; the other had been sulphate of baryta, but by long continued boiling with much carbonate of potash, it had suffered the opposite change into the carbonate. The Lecturer then stated that so great is the influence exerted by these various circumstances, that some have doubted whether there be a true "elective affinity;" he however believed that after making every allowance for known causes there is still a residuary phenomenon to which that name is the most appropriate. Allowing then, with Bergmann, that relative degrees of affinity exist, the question arises:—Is Berthollet's law also correct? It is very difficult to arrive at a satisfactory answer, since it is almost impossible to eliminate other influences. Several reactions, however, were mentioned as tending to show that there is some truth in the law:—for instance, the solution of gold in hydrochloric acid upon the addition of nitrate of potash. The experiments of Bunsen on mixtures of carbonic oxide and hydrogen, exploded with a quantity of oxygen insufficient for complete combustion; and those of Debus on the precipitation of mixed hydrates of lime and baryta by carbonic acid, were explained; as also the remarkable fact noticed by both, that the resulting products were always in certain atomic proportions to one another. But in both these cases the first products of the chemical action are removed at once from the field: it is quite another case when they remain free to act and react on one another. Supposing they all remain in solution, the requisite is fulfilled; but how are we to know what has then taken place? Malaguti thought to obtain an indication of this by mixing the aqueous solutions of two salts, one of which is soluble in alcohol, and the other is insoluble, and then pouring them into very strong alcohol, and analyzing the salts immediately thrown down. His results are tabulated; they are valuable, but to some extent open to objection on account of the disturbing influence of the alcohol. Some observations of Professor Graham, and others of Professor Williamson, as yet unpublished, were then spoken of, and the Lecturer proceeded to describe his own endeavours to arrive at a knowledge of the intimate constitution of a mixture of salts in solution by observing their physical properties, especially colour.

If solutions of one equivalent of nitrate of iron, and a triple equivalent of sulphocyanide of potassium be mixed, a blood-red colour results owing to the formation of sulphocyanide of the sesquioxide of iron; the question arises—Has all the iron left the nitric acid to unite itself with the sulphocyanogen? It has not; for on the addition of equivalent after equivalent of sulphocyanide of potassium, a deeper red is constantly obtained. The arrangement by which this deepening of colour was quantitatively determined was explained, and imitated on the lecture table. The result was that even up to 375 equivalents, a regular increase was observed to take place, more rapidly at first than afterwards, which was exhibited to the eye by

the results being projected as a curve. Again, as in the mixture of equal equivalents of the two salts, some iron still remains in combination with the nitric acid, a portion of the potassium must still remain united to the sulphocyanogen. Accordingly, the addition of more iron salt also gives a deeper colour. The curve expressing the results of this experiment was a regular continuation of the curve formerly mentioned; and neither of them exhibited any of those sudden transitions which the experiments of Bunsen and Debus present. Diagrams exhibiting curves of the gallate and meconate of iron were also exhibited. Various experiments were then performed, showing the alteration in the resulting colour upon any change of any of the elements in the primary experiment; for instance, the substitution of other acids for the nitric acid, or of other bases for the potash. On the addition of a colourless salt to a coloured one, there results a diminution of the colour greater than the mere dilution would have produced, as was exemplified in the cases of the red sulphocyanide of iron mixed with sulphate of potash, and of the scarlet bromide of gold mixed with chloride of potassium. The lecturer accordingly drew the conclusion that when two salts mix without precipitation or volatilization, the acids and bases frequently, if not universally, arrange themselves according to some definite proportion; and that this depends on the relative quantity of the two salts, as well as upon the proper affinities of the substances composing them. He was unable then to enter upon the influence of heat, or of dilution in certain cases, or to add any remarks connected with double salts, or with other metals, or upon certain practical applications of these views in chemical and physiological science.

The fact that we very frequently find the double decomposition of a salt to be *complete*, the *whole* of one of its constituents being precipitated, was shown to be easily explained on the principles of Berthollet. Thus, for instance, when chromate of potash and nitrate of silver are mixed, at the first moment a division will take place producing four salts, but one of these—the chromate of silver, is thrown down at once as a precipitate, and thus put out of the field of action. Another division of the acids with the bases must take place, producing of course more of the insoluble chromate, and so on till at length the whole of the silver is removed. And that this is really what does take place is rendered almost certain by the fact that wherever by an interchange of acids and bases a precipitate can be produced, that precipitate does form; and, if the substance be perfectly insoluble, the whole is thrown down; this occurring in opposition to all rules of “affinity,” and to all tables that Bergmann, or any other chemist, ever did or could construct. The volatility of one of the products acts in the same manner as insolubility, as is exemplified in the decomposition of carbonates by any other acid. Crystallization also is but another phase of the same phenomenon. An experiment was exhibited in illustration of this. Dilute solutions of nitrate of lime, and sulphate of soda, were mixed

at the ordinary temperature without producing any separation of solid matter; but they were so proportioned that upon heating the mixture, the crystallization of some sulphate of lime was determined, and when once this had commenced, it progressed rapidly; resembling in that respect the ordinary phenomena of precipitation. If in a double decomposition a far larger quantity of a sparingly soluble salt be produced at the first moment than the water can dissolve, the crystals will be formed rapidly and will accordingly be very small in size; but should there be formed at once only just sufficient to determine a separation in the solid form, the crystals will grow gradually, and will often attain a large size. This was exemplified on the mixture of nitrate of silver with the sulphates of copper and of potash respectively.

It is possible that the law of Berthollet may not be universally applicable; yet the present advanced state of science shows that not only is there, as Bergmann insisted, a true chemical affinity, that is—a preference of one substance to combine with a certain other substance instead of a third,—but, in a great number of instances at least, this substance will combine with both according to certain proportions, whenever the whole of the affinities can be brought into play at the same time.

[J. H. G.]

Friday, May 19.

W. R. GROVE, Esq., Q.C., F.R.S., Vice-President, in the Chair.

JOHN TYNDALL, Esq., Ph.D., F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY IN THE ROYAL INSTITUTION.

On some Phenomena connected with the Motion of Liquids.

THE Lecturer commenced by referring to certain phenomena exhibited by liquids, and at variance with our commonly received notions as to their non-cohesive character. According to Donny, when the air has been as far as possible expelled from water by persistent boiling, such water possesses an extraordinary cohesive power, sufficient indeed to permit of its being heated to a temperature of 275° Fah. without boiling. The adhesion of water thus prepared to the surface of a glass tube was shewn experimentally; the force being sufficient to sustain a column of water of considerable height. The contractile force of a soap-bubble was referred to; and the Lecturer passed on to the exhibition of the phenomena resulting from the shock of two opposing liquid veins. In this case, though the forces are in opposite directions, motion is not annihilated; but the liquid, as first shewn by Savart, spreads out so as to form a thin transparent film, the plane of which is at right angles to the direction of the jets. By varying the pressure on one side or the other, or by making the jets of different diameters, the plane film could be converted into a curved one, and sometimes actually caused to close, so as to form a pellucid sack. A cistern, situated at the top of the house and communicating by pipes with the lecture table, placed a considerable pressure at the disposal of the Lecturer, and enabled him to exhibit in a striking manner the various phenomena described by Savart in his researches on the motion of liquids. A vein was caused to fall vertically upon a brass disk upwards of three inches in diameter: the liquid spread laterally on all sides and formed an umbrella-shaped pellicle of great size and beauty. With a disk of an inch in diameter, a pellicle of at least equal magnitude was formed. When a candle was placed underneath the curved sheet of water a singular effect was produced. The film above the candle was instantly dissipated; and on moving the candle, its motion was followed by a corresponding change of the aqueous surface. On turning a suitable cock so as to lessen the pressure, the

curvature of the film became increased, until finally the molecular action of the water caused it to form a curve returning upon itself, and exhibiting the appearance of a large flask. When the film completely embraced the vertical stem which supported the brass disk, a change in the form of the liquid flask was observed, the latter became elongated, and was sometimes divided into two portions, one of which glided down the vertical stem and was broken at its base. When the jet was projected vertically upwards, large sheets were also obtained. The jet was also suffered to fall into small hollow cones of various apertures, and the shape of the liquid sheet received thereby some beautiful modifications. The inclosed sides of the hollow cone gave the liquid an ascending motion which, combined with the action of gravity, caused the film to bend and constitute a vase-shaped surface of great beauty. The Lecturer next referred to the constitution of a liquid vein; he had pointed out, some years ago, a simple mode of observing this constitution by means of the electric spark; this method corroborated the result before arrived at by Savart, that the lower portion of a liquid vein owes its turbidity to the fact of the mass being there reduced to drops, although the quickness with which they succeed each other gives the eye the impression of continuity. Savart's last experiments on this subject were repeated: a tube about five feet long and two inches wide had a perforated brass disk fixed at its lower extremity; the tube was filled with water, which, after it had become motionless, was permitted to issue from an orifice pierced in the centre of the disk. As the liquid escaped it gave birth to a succession of musical notes of sufficient intensity to be distinctly heard throughout the theatre. That these notes were not due to the motion imparted to the air by the descending drops of the liquid vein was proved, first, by intercepting the vein in its continuous portion, and secondly, by permitting it to discharge itself into a vessel containing water, the orifice being caused to dip beneath the surface of the latter. In this case the mass of liquid was continuous, but the notes were nevertheless produced; thus shewing that the vibrations which produce them must take place in the glass cylinder itself; — and corroborating the conclusions arrived at by Savart from his earliest experiments on this subject. The pitch of the note depends upon the height of the liquid column which produces it; and by attaching a tube of an inch in diameter, furnished with a perforated bottom, to a cylindrical vessel about eighteen inches wide, and filling the whole with water, a note of long duration and of sensibly constant pitch was obtained.

The Lecturer concluded with an experimental illustration of the total reflexion of light at the common surface of two media of different refractive indices. The tube communicating with the reservoir before referred to was fitted into the top of a small

box, into one of the sides of which was fitted a glass tube three quarters of an inch wide and five inches long. The side of the box opposite to that through which the glass tube was introduced was of glass. Behind the box was placed a camera, by means of which the electric light could be condensed and caused to pass, first through the glass back of the box, and then through the tube in front, so as to form a white disk upon a screen held in the direct path of the light. When, however, the cock was turned so as to permit water to spout from the tube, the light on reaching the limiting surface of air and water was totally reflected, and seemed to be washed downward by the descending liquid, the latter being thereby caused to present a beautiful illuminated appearance.

[J. T.]

Friday, May 26,

SIR HENRY HOLLAND, Bart., M.D., F.R.S., Vice-President,
in the Chair.

B. C. BRODIE, Esq., F.R.S.

On Melting Points.

WHEN the temperature of certain substances is raised, they pass from the solid to the fluid and from the fluid to the gaseous condition. These transitions are attended with the absorption of heat. There are other bodies which by elevation of temperature undergo a transformation of a different kind. Thus, when liquid phosphorus is heated, in such a manner that its change into the gaseous condition is prevented, at a certain temperature it becomes solid, and passes into the red modification; these allotropic changes also are invariably attended with evolution or absorption of heat.

Considerable anomalies are found in the statements which different experimenters have made as to the melting point of sulphur. The cause of these discrepancies lies in the facility with which the allotropic condition of sulphur is altered by heat. The melting point of octohedral sulphur lies very close upon the point at which it undergoes a change into the oblique-prismatic condition. When this sulphur has been melted, it passes more or less completely into a third allotropic form. For these reasons, the melting point taken was never that of a *pure* sulphur. However, by certain precautions in experimenting, the true melting points of sulphur have been ascertained.

The experiment cannot be made in the usual manner of taking a melting point, namely, by placing a thermometer in the fluid substance, and observing the point of solidification. Fluid sulphur is always a mixture of more than one modification. The experiment is made by placing minute fragments of sulphur in thin glass-tubes, immersing the tubes in a bath of dilute sulphuric acid, and observing the temperature of the fluid at the melting of the substance. Experiments thus conducted have shewn that the melting point of octohedral sulphur is 114.5°C . and of the oblique sulphur 120°C . This latter sulphur is obtained in a pure condition by heating the octohedral sulphur at a temperature below its melting point, from 100° to 110°C . This change invariably takes place when the sulphur is exposed, even momentarily, to this temperature in a state of powder.

The solidifying point of melted sulphur varies according to the

temperature to which it has been raised in the melted condition. Powdered sulphur, carefully melted so as not to raise its temperature above one degree beyond its melting point, will solidify precisely at its melting point, 120°C . If, however, the temperature be raised to 300°C . it will solidify at about 110°C . The cause of this difference is, that the sulphur in the latter case always contains a large portion of a third modification, namely, the viscid form of sulphur.

There are some remarkable anomalies in melting points which do not so readily as the above admit of explanation. Under certain circumstances drops of sulphur will remain in the liquid condition at a temperature far below the true point of solidification, and solidify instantaneously when touched. The same is the case with phosphorus. Water contained in a capillary tube may be immersed without freezing in a mixture cooled to -110°C . The same experiment may be made with a considerable quantity of water if the surface be protected by a thin layer of æther. In these cases the water instantly freezes by agitation or by touching the surface with a solid body. Similar observations have been made in the crystallization of certain salts. A solution of sulphate of soda made at 30° or 40°C . will not crystallize on cooling, provided the fluid be not disturbed, but instantly crystallizes on touching the surface with a wire. This phenomenon does not take place with all solutions. A solution of nitre crystallizes normally. There is also a difference of degree in this property. A solution of borax will remain in an open flask in the supersaturated condition, and crystallizes only on violent agitation.

An experiment was shewn by which a connexion was established between this class of facts and those of which mention was first made. Two tubes were exhibited, each containing the same quantity of sulphur dissolved in the same quantity of bisulphide of carbon. One tube had crystallized in the normal manner, the other had deposited no crystals. The sulphur had in both tubes been dissolved at the same time and in the same manner. But the tube in which the sulphur did not crystallize had been exposed to a higher temperature than the other tube. In this case therefore it was evident that the cause of the supersaturated condition was an alteration of the substance induced by heat. On breaking the point of the tube and agitating the fluid with a wire, the sulphur instantly crystallized. The analogy was pointed out of the sudden alteration of this condition by agitation and contact, and the decomposition which many chemical substances, such as the iodide of nitrogen, undergo by similar causes.

[B. C. B.]

Friday, June 2.

WILLIAM ROBERT GROVE, Esq., Q.C., F.R.S., Vice-President,
in the Chair.

DR. E. FRANKLAND, F.R.S.

*On the dependance of the Chemical properties of Compounds upon the
Electrical character of their constituents.*

THE Lecturer first directed attention to the remarkable continuity and correlation of the natural forces, owing to which, the philosopher, seeking to eliminate the effects legitimately due to each, frequently experienced the greatest difficulty in separating the true results of a single force, from the cognate influence of other forces. Such difficulties were more especially encountered in the manifestations of the chemical force or chemical affinity, which rarely or never acted singly and alone, but was constantly accompanied, modified, and controlled, by collateral forces, which alternately exalted, depressed, or altogether inverted it.

The powerful influence of cohesion and heat especially attracted the attention of Berthollet, and so impressed that profound philosopher with their potency, as to lead him to ignore completely the existence of a separate chemical force. Notwithstanding the otherwise singularly ingenious and sound conclusions of this chemist, the Lecturer believed that later researches had demonstrated the total denial of a distinct chemical force to be untenable.

The influence of electricity upon chemical affinity was perhaps even still greater than that of cohesion or heat; the most powerful combinations being broken up by this agent, if its operations were favoured by the two conditions — mobility of particles (fluidity), and conductivity of the electric current. The phenomenon of the evolution of the separate elements of a binary compound, at the opposite poles of the decomposing cell, was one of the most remarkable attending the resolution of compounds into their elements by the electrical force. It immediately attracted the attention of philosophers, and almost forced upon them the conclusion, that such elements were oppositely electrified.

Davy was the first to seize upon these facts and model them into an electro-chemical theory, which, notwithstanding its defects, was at least as soundly philosophical as those which succeeded it. Davy supposed that the elements in their uncombined condition did not contain free electricity, but that by contact they

became excited. Thus, a particle of sulphur became negative when placed in contact with a particle of copper, which last was simultaneously rendered positive: the application of heat intensified the charge, until at a certain point, the tension of the two electricities became so high, that they suddenly re-combined, carrying with them the molecules of copper and sulphur, which were thus intimately mingled, whilst evolution of heat and light resulted from the combination of the two electricities. Ampère and Berzelius subsequently attempted to remove some of the difficulties, which were encountered in endeavouring to make Davy's theory embrace all chemical phenomena. Ampère considered each element to be permanently endowed with a definite amount of one or the other electricity, being thus invariably either electro-positive or electro-negative to an extent dependent upon the intensity of the charge. Such a naturally charged molecule Ampère imagined to attract around it an atmosphere of the opposite electricity of corresponding intensity, and that when two molecules oppositely charged were brought in contact, their atmospheres of electricity united, giving rise to the heat and light of chemical combination, whilst the original charge retained the attracting molecules in permanent union. Although this theory elucidated some points which Davy's view left unexplained, yet it would not be difficult to start several very serious objections to it: the attempted removal of these gave rise to the electro-chemical theory of Berzelius, who supposed that each element contained the two electricities, but that the one was more powerfully developed than the other, as in the case of a magnet in which one pole, by being divided, was apparently weaker than the other. In chemical combination, Berzelius imagined, that one of the electricities of each element was discharged, producing the heat and light of chemical action, whilst the other was retained and served to hold the elements in combination.

But these attempts of Ampère and Berzelius to improve the theory of Davy succeeded perhaps less in perfecting our views of electro-chemical phenomena, than in demonstrating the necessity for much further research, before these phenomena could be satisfactorily interpreted; for these theories, in which different degrees of affinity were explained by differences in the degree of electrical excitement, have been proved radically defective by the remarkable discovery of Professor Faraday, that compounds, whose elements were united by the most dissimilar degrees of affinity, required equal quantities of electric force for their decomposition.

Such defects in the attempts to account for chemical phenomena by electrical agency led Dumas and other chemists to reject altogether the idea of electro-chemical combination. Dumas regarded a chemical compound as a group of molecules connected by a single force in a manner analogous to a planetary system, and the chemical character of a compound as dependant upon the position

of the separate molecules, and not upon their individual character. This beautiful and highly poetical view would neither have received such an extensive adoption, nor have been the parent of such numerous and brilliant discoveries in the organic portion of the science, if it had not contained a profound truth: nevertheless the Lecturer conceived that the total abnegation of the influence of the electrical character of elements upon the chemical properties of their compounds, implied by this theory of types, was directly opposed to many of the phenomena of chemical combination, which invariably revealed such a connection.

The effect of successive additions of oxygen to an electro-positive element, in gradually weakening its basic, and consequently electro-positive, qualities, and finally converting it into an acid, or electro-negative body, was well known in the case of manganese, iron, chromium, gold, &c., but the effects of the juxtaposition of two or more elements of similar electrical character had not hitherto been much studied. Granting the existence of an electrical charge associated with the molecules of matter, it was evident that such a union of atoms, as that just mentioned, would resemble two approximated globes similarly electrified. Now the effect of the approximation of two such globes would be the intensification of the charge of each; and therefore, if there were any connection between electrical and chemical character, it would be exemplified by an increased energy of affinity under such circumstances. Examples of such an approximation of atoms of similar character were not wanting, even amongst inorganic bodies: thus the compounds of chlorine with oxygen were remarkable instances of the union of like atoms; and we saw in several of them the truth of the foregoing proposition fully borne out. Hypochlorous, chlorous, and chloric acids were all distinguished by the intense energy of their affinities and contrasted strongly with the compounds of oxygen or chlorine with electro-positive elements.

The compounds of phosphorus with hydrogen also exemplified the same effect. Phosphorus, though usually regarded as an electro-negative body, was yet far more closely associated in its general character with the metals than with the metalloids; we were therefore entitled to regard a compound of this element with hydrogen, as a juxtaposition of two similarly electrified atoms. Now two of the compounds of phosphorus with hydrogen, viz. bin-hydride and ter-hydride of phosphorus, were remarkable for the intensity of their affinities, the one being spontaneously inflammable and the other merely requiring a diminution of pressure, when mixed with atmospheric air or oxygen, to determine its combustion.

But the influence of the electrical character of elements upon the chemical properties of their compounds was perhaps most strikingly seen in the behaviour of the organo-metallic bodies, nearly all of which had only recently been discovered. Most of these bodies, which, in their isolated condition, consisted of two or more simi-

larly electrified atoms, were distinguished by an intensity of affinity which was quite foreign to their proximate, or even elementary, constituents. Zinc and methyl, for instance, were neither of them distinguished for any remarkable energy of affinity in their free state; but united as zinc-methylum, they formed a compound whose combining energy surpassed that of all known bodies, and this behaviour was shared in also by the corresponding compounds of zinc with ethyl and amyl. In cacodyl, stanethylum, stibethylum, and the new compounds of arsenic with ethyl, we had additional and striking evidence of the same law, for the affinities of arsenic, tin, and antimony, were, in these compounds, exalted in a most remarkable manner by the approximation of similarly electrified atoms.

These examples seemed to prove clearly the great influence of the electrical character of elements upon the chemical properties of their compounds; but further study of the subject also revealed the paramount influence of molecular structure, which modified and controlled the effects of electrical character, and limited all affinity however heightened by electric induction. To this effect of molecular arrangement was no doubt to be attributed the occurrence of some apparent anomalies which, at first sight, appeared to contradict the general law just laid down, such as perchloric acid, biphosphide of hydrogen, &c.; but the pursuit of the subject into this ramification would have far exceeded the limits of the lecture, the chief object of which was to point out that, although all the electro-chemical theories hitherto proposed were far from satisfactory, yet, that amongst the factors of chemical action, the electrical character of elements could not be denied a place, without ignoring and leaving unexplained some of the most remarkable of chemical phenomena.

[E. F.]

Friday, June 9.

SIR HENRY HOLLAND, Bart., M.D., F.R.S., Vice-President,
in the Chair.

PROFESSOR FARADAY, D.C.L., F.R.S.

On Magnetic Hypotheses.

THIS discourse, the purpose of which was to direct the attention of the audience to the different hypothetical attempts made to account physically for the known properties of matter in relation to its magneto-electrical phenomena, followed on very naturally to that of Dr. Frankland on the 2nd instant, who then gave an account of the different views advanced by Davy, Ampère, and Berzelius, of the manner in which electricity might be associated with the atoms or molecules of matter, so as to account for their electro-chemical actions, and of the logical and experimental objections which stood in the way of each. On the present occasion reference was first made to Coulomb's investigations of mutual magnetic actions; to the hypothesis advanced by him, that two magnetic fluids, associated with the matter of magnetic bodies, would account for all the phenomena; and to Poisson's profound mathematical investigation of the sufficiency of the hypothesis. Then Oersted's discovery of the relation of common magnetism to currents of electricity was recalled to mind:—hence an enormous enlargement of the scope of magnetic force and of our knowledge of its actions; and hence Ampère's beautiful investigations, and his hypothesis (also sustained by the highest mathematical investigation),—that all magnetic phenomena are due to currents of electricity; and that in such bodies as magnets, iron, nickel, &c. the atoms or particles have naturally currents of electricity running round them in one direction, about what may be considered as their equatorial parts. After Oersted's time, further experimental discoveries occurred; currents of electricity were found competent to induce collateral currents, and magnets proved able to produce like currents; thus shewing the identity of action of magnets and currents in producing effects of a kind different to ordinary magnetic attractions and repulsions. Then diamagnetism was discovered, in which actions analogous to those of ordinary magnetism occurred, but with the antithesis of attraction for repulsion and repulsion for attraction: and these were so extensive, that whatever bodies were not magnetic proved to be diamagnetic; and thus *all* matter was brought under the dominion of that magnetic force, whose physical mode of action

hypothesis endeavours to account for. As the hypothesis of Ampère could not account for diamagnetic action, some assumed that magnetic and electric force might, in diamagnetic matter, induce currents of electricity in the reverse direction to those in magnetic matter; or else might induce currents where before there were none: whereas in magnetic cases it was supposed they only constrained particle-currents to assume a particular direction, which before were in all directions. Weber stands eminent as a profound mathematician who has confirmed Ampère's investigations as far as they proceeded, and who has made an addition to his hypothetical views; namely, that there is electricity amongst the particles of matter, which is not thrown into the form of a current until the magnetic induction comes upon it, but which then assumes the character of current, having a direction the contrary to that of the currents which Ampère supposed to be always circulating round magnetic matter; and so these other matters are rendered diamagnetic.

De la Rive, who has recently most carefully examined the various hypotheses, and who as an experimentalist and discoverer has the highest right to enter into the consideration of these deep, searching, and difficult enquiries, after recalling the various phenomena which shew that the powers concerned belong to the particles of matter and not to the masses merely, (the former conferring them by association upon the latter,) then distinguishes magnetic action into four kinds or modes,—namely, the ordinary, the diamagnetic, the induction of currents, and the rotation of a ray; and points out that any acceptable hypotheses ought to account for the *four* modes of action, and, it may be added, ought to agree with, if not account for, the phenomena of electro-chemical action also. De la Rive conceives that as regards these modes of action this hypothetical result may be obtained, and both Ampère and Weber's views also retained in the following manner. All the atoms of matter are supposed to be endowed with electrical currents of a like kind, which move about them for ever, without diminution of their force or velocity, being essentially a part of their nature. The direction of these currents for each atom is through one determinate diameter, which may therefore be considered as the axis. Where they emerge from the body of the atom they divide in all directions, and running over every part of the surface converge towards the opposite end of the axis diameter, and there re-enter the atom to run ever through the same course. The converging and diverging points are as it were poles of force. Where the atoms of matter are close or numerous in a given space, (and chemical considerations lead to the admission of such cases,) the hypothesis then admits that several atoms may conjoin into a ring, so that their central or axial currents may run one into the other, and not return as before over the surface of each atom: these form the molecules of magnetic matter, and represent Ampère's hypothesis of molecular currents.

Where the atoms, being fewer in a given space, are farther apart, or where, being good conductors, the current runs as freely over the surface as through the axis, then they do not form like groups to the molecules of magnetic matter, but are still considered subject to a species of induction by the action of external magnets and currents; and so give rise to Weber's reverse currents. The induction of momentary currents and the rotation of a ray are considered by De la Rive as in conformity with such a supposition of the electric state of the atoms and particles of matter.

The Lecturer seemed to think that the great variety of these hypotheses and their rapid succession was rather a proof of weakness, in this department of physical knowledge than of strength, and that the large assumptions which were made in turn for each should ever be present to the mind. Even in the most perfect of them, *i. e.* De la Rive's, these assumptions are very considerable; for it is necessary to conceive of the molecules as being flat or disc-like bodies, however numerous the atoms of each may be; also that the atoms of one molecule do not interfere with or break up the disposition of those of another molecule; also that electro-chemical action may consist with such a constituted molecule; also that the motive force of each atom current is resident in the axis, and on the other hand that the passage of the current over the surface offers *resistance*; for unless there were a difference between the axial and the surface force in one direction or the other, the atoms would have no tendency to congregate in molecules. In making these remarks, however, the speaker had no thought of depreciating hypothesis or objecting to its right use. No discoverer could advance without it; and such exertions as those made by De la Rive, to bring into harmony thoughts which in their earlier forms were adverse to each other, were of the more value, because they were the exertions of a man who knew the value both of hypothesis and of laws, of theory and of fact, and had given proofs of the power of each by the productions of his own mind. Still the speaker advocated that mental reservation which kept hypothesis in its right place and which was ready to abandon it when it failed; and as examples referred to Newton, who (as is shewn by his Letters to Bentley) had very strong convictions of the physical nature of the lines of gravitating force, yet in what he publicly advanced stopped short at the law of action of the force, and thence deduced his great results;—and also to Arago, who, discovering the phenomena of magnetic rotation, yet not perceiving their physical cause, had that philosophic power of mind which enabled him to refrain from suggesting one.

[M. F.]

Friday, January 19.

WILLIAM ROBERT GROVE, Esq. Q.C. F.R.S. Vice-President,
in the Chair.

PROFESSOR FARADAY, D.C.L. F.R.S.

On some points of Magnetic Philosophy.

THE magnetic and electric forms of power, being dual in their character, and also able to act at a distance, will probably aid greatly in the development of the nature of physical force generally: and if (as I believe) the dualities are essential to the forces, are always equal and equivalent to each other, and are so mutually dependent, that one cannot appear, or even exist, without the other, the proof of the truth of such conditions would lead to many consequences of the highest importance to the philosophy of force generally. A few brief experiments with the electric power quickly place the dual cases before the contemplative mind. Thus, if a metallic vessel, as an ice-pail, be insulated and connected with a delicate gold leaf electrometer, or other like instrument, and then an insulated metallic globe, half the diameter of the ice-pail, be charged with positive electricity and placed in the middle of the pail, the latter being for the moment uninsulated by a touch outside, and then left insulated again, the whole system will show no signs of electricity externally, nor will the electrometer be affected: but a carrier applied to the ball within the vessel will bring away from it positive electricity, showing its particular state of charge; or being applied to the lower inside surface of the vessel will bring away negative electricity, proving that it has the contrary state: or the duality may be proved by withdrawing the ball, when the vessel will show itself negative by the electrometer, and the ball will be found positive. That these dualities are equal, is further shown by replacing the ball within the vessel, observing the electrometer, bringing the ball and vessel in contact, and again observing the electrometer, which will remain unchanged; and finally withdrawing the ball, which comes away perfectly discharged, and leaves the vessel externally in its unchanged and previous state. So the electric dualities are equal, equivalent, and mutually sustained. To show that one cannot exist alone, insulate the metallic vessel, charge it strongly by contact with the machine or a Leyden jar, and then dip the insulated ball into it; and after touching the bottom of the vessel with the ball, remove it, without touching the sides: it will be found

absolutely free from charge, whatever its previous state may have been ; for none but a single state can exist at the bottom of such a metallic vessel ; and a single state, *i.e.* an unrelated duality, cannot exist alone.

The correspondent dualities, *i.e.* the northness and the southness, of the magnetic force are well known. For the purpose of insulating, if possible, one of these, and separating it in any degree from the other, numerous experiments have been made. Thus six equal electro-magnets, formed of square bars, were put together in the direction of three lines perpendicular to each other, so that their inner ends, being all alike in polarity, might inclose a cubical space and produce an experimental chamber. When excited, these magnets were very powerful in the outer direction, as was found by nails, filings, spirals, and needles ; but within the chamber, walled in on every side by intense north poles, there was no power of any kind : filings were not arranged ; small needles not affected, except as they by their own inducing powers caused arrangement of the force within ; revolving wire helices produced no currents : the chamber was a place of no magnetic action. Ordinary magnetic poles of like nature produced corresponding results. A single pole presented its usual character, attracting iron, repelling bismuth ; a like pole, at right angles to it, formed a re-entering angle, and there a weak place of magnetic action was caused ; iron was attracted from it to the prominent corners ; bismuth moved up into it ; and a third like pole on the opposite side made the place of weak force still weaker and larger ; another pole or two made it very weak ; six poles brought it to the condition above described. Even four poles, put with their longer edges together, produced a lengthened chamber with two entrances : and a little needle being carried in at either entrance passed rapidly through spaces of weaker and weaker force, and found a part in the middle where magnetic action was not sensible.

Other very interesting results were obtained by making chambers in the polar extremities of electro-magnets. A cylinder magnet, whose core was 1.5 inches in diameter, had a concentric cylindrical chamber formed in the end, 0.7 in diameter, and 1.3 inches deep. When iron filings were brought near this excited pole, they clung around the outside, but none entered the cavity, except a very few near the outer edge. When they were purposely placed inside on a card they were quite indifferent to the excited pole, except that those near the mouth of the chamber moved out and were attracted to the outer edges. A piece of soft iron at the end of a copper wire was strongly attracted by the outer parts of the pole, but unaffected within. When the chamber was filled with iron filings and inverted, the magnet being excited, all those from the bottom and interior of the chamber fell out ; many, however, being caught up by the outer parts of the pole. If pieces of iron, successively increasing from the size of a filing to a nail, a spike, and so on to a

long bar, were brought into contact with the same point at the bottom of the inverted chamber, though the filing could not be held by attraction, nor the smaller pieces of iron, yet as soon as those were employed which reached to the level of the chamber mouth, or beyond it, attraction manifested itself; and with the larger pieces it rose so high that a bar of some pounds weight could be held against the very spot that was not sufficient to retain an iron filing.

These and many other results prove experimentally, that the magnetic dualities cannot appear alone; and that when they are developed they are in equal proportions and essentially connected. For if not essentially connected, how could a magnet exist alone? Its power, evident when other magnets, or iron, or bismuth is near it, must, upon their removal, then take up some *other form*, or exist *without action*: the first has never been shown or even suspected; the second is an impossibility, being inconsistent with the conservation of force. But if the dualities of a single magnet are thrown upon each other, and so become mutually related, is that in right lines through the magnet, or in curved lines through the space around? That it is not in right lines through the magnet (it being a straight bar or sphere) is shown by this, that the proper means as a helix round the magnet, shows that the *internal* disposition of the force (coercitive or other) is not affected when the magnet is exerting its power on other magnets, or when left to itself (*Experimental Researches*, 3119, 3121, 3215, &c.); and like means show that the *external* disposition of the force is so affected: so that the force in right lines through the magnet does not change under the circumstances, whilst the force in external (and necessarily) curved lines does.

The polarity of bismuth or phosphorus in the magnetic field is one point amongst many others essentially dependent upon, and highly illustrative of the nature of, the magnetic force. The assumption that they have a polarity the reverse of that of paramagnetic bodies involves the consequence, that northness does not always repel northness or attract southness; or else leads to the assumption that there are two northnesses and two southnesses, and that these sometimes associate in pairs one way, and at other times in the contrary way. But leaving the assumptions and reverting to experiment, it was hoped that a forcible imitation of the imagined state of bismuth in the magnetic field, might illustrate its real state, and, for this purpose, recourse was had to the indications given by a moving conductor. Four spheres of copper, iron, bismuth, and hard steel have been prepared, and rotated upon an axis coincident with the magnetic axis of a powerful horse-shoe magnet; each sphere has a ring of copper fixed on it as an equator, and the ends of a galvanometer wire were brought into contact with the axis and the equator of the revolving globe. Under these circumstances, the electric current produced in the moving globe

was conveyed to the galvanometer, and became the indicator of the magnetic polarity of the spheres; the direction of rotation, and the poles of the magnet, being in all cases the same. When the copper sphere, as a standard, was revolved, deflection at the galvanometer occurred in a certain direction. When the iron sphere replaced the copper and was revolved, the deflection at the galvanometer was the same. When the bismuth sphere was employed, the deflection was still the same:—and it still remained the same when the steel sphere was rotated in the magnetic field. Hence, by this effect, which I believe to be a truthful and unvarying indication of polarity, the state of all the spheres was the same, and therefore the polarity of the magnetic force in the iron, copper, and bismuth, in every case alike. (*Exp. Res.* 3164, &c.) The steel sphere was then magnetized in the direction of its axis, and was found to be so hard as to retain its own magnetic state when in a reverse direction between the poles of the dominant magnet, for upon its removal its magnetism remained unchanged. Experiments were then made in a selected position, where the dominant magnetic force was not too strong—(a magnet able to lift 430 lbs. was used)—and it was found that when the steel magnet was placed in accordance, *i.e.* with its north pole opposite the south pole of the dominant magnet, the deflection was in the same direction as with the bismuth sphere; but when it was changed so as to be in the magnetic condition assigned by some to bismuth (*i.e.* with reversed polarities), it then differed from bismuth, producing the contrary deflection. [For a further account of these considerations and investigations, a paper may be referred to, which will appear in the February number of the *Philosophical Magazine*.]

It is, probably, of great importance that our thoughts should be stirred up at this time to a reconsideration of the general nature of physical force, and especially to those forms of it which are concerned in actions at a distance. These are, by the dual powers, connected very intimately with those which occur at insensible distances; and it is to be expected that the progress which physical science has made in latter times will enable us to approach this deep and difficult subject with far more advantage than any possessed by philosophers at former periods. At present we are accustomed to admit action at sensible distances, as of one magnet upon another, or of the sun upon the earth, as if such admission were itself a perfect answer to any enquiry into the nature of the physical means which cause distant bodies to affect each other; and the man who hesitates to admit the sufficiency of the answer, or of the assumption on which it rests, and asks for a more satisfactory account, runs some risk of appearing ridiculous or ignorant before the world of science. Yet Newton, who did more than any other man in demonstrating the law of action of distant bodies, including amongst such the sun and Saturn, which are 900 millions of miles apart, did not leave the subject without

recording his well-considered judgment, that the mere attraction of distant portions of matter was not a sufficient or satisfactory thought for a philosopher. That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance through a vacuum, without the mediation of anything else, by and through which their action and force may be conveyed from one to another, is, he says, to him a great absurdity. Gravity must be caused by an agent, acting constantly according to certain laws; but whether this agent be material or immaterial he leaves to the consideration of his readers. This is the onward looking thought of one, who by his knowledge and like quality of mind, saw in the diamond an unctuous substance coagulated, when as yet it was known but as a transparent stone, and foretold the presence of a combustible substance in water a century before water was decomposed or hydrogen discovered: and I cannot help believing that the time is near at hand, when his thought regarding gravity will produce fruit:—and, with that impression, I shall venture a few considerations upon what appears to me the insufficiency of the usually accepted notions of gravity, and of those forces generally, which are supposed to act at a distance, having respect to the modern and philosophic view of the conservation and indestructibility of force.

The notion of the gravitating force is, with those who admit Newton's law, but go with him no further, that matter attracts matter with a strength which is inversely as the square of the distance. Consider, then, a mass of matter (or a particle), for which present purpose the sun will serve, and consider a globe like one of the planets, as our earth, either created or taken from distant space and placed near the sun as our earth is;—the attraction of gravity is then exerted, and we say that the sun attracts the earth, and, also, that the earth attracts the sun. But if the sun attracts the earth, that force of attraction must either arise *because* of the presence of the earth near the sun; or it must have *pre-existed* in the sun when the earth was not there. If we consider the first case, I think it will be exceedingly difficult to conceive that the sudden presence of an earth, 95 millions of miles from the sun, and having no previous physical connexion with it, nor any physical connexion caused by the mere circumstance of juxtaposition, should be able to raise up in the sun a power having no previous existence. As respects gravity, the earth must be considered as inert, previously, as the sun; and can have no more inducing or affecting power over the sun than the sun over it: both are assumed to be *without* power in the beginning of the case;—how then can that power arise by their mere approximation or co-existence? That a body without force should raise up force in a body at a distance from it, is too hard to imagine; but it is harder still, if that can be possible, to accept the idea when we consider that it includes the *creation of force*. Force may be opposed by force, may be diverted,

directed partially or exclusively, may even be converted, as far as we understand the matter, disappearing in one form to reappear in another ; but it cannot be created or annihilated, or truly suspended, *i.e.* rendered existent without action or without its equivalent action. The conservation of power is now a thought deeply impressed upon the minds of philosophic men ; and I think that, as a body, they admit that the creation or annihilation of force is equally impossible with the creation or annihilation of matter. But if we conceive the sun existing alone in space, exerting no force of gravitation exterior to it ; and then conceive another sphere in space having like conditions, and that the two are brought towards each other ; if we assume, that by their mutual presence each causes the other to act,—this is to assume not merely a creation of power but a *double creation*, for both are supposed to rise from a previously inert to a powerful state. On their dissociation they, by the assumption, pass into the powerless state again, and this would be equivalent to the *annihilation* of force. It will be easily understood, that the case of the sun or the earth, or of any one of two or more acting bodies, is reciprocal ;—and also that the variation of attraction, with any degree of approach or separation of the bodies, involves the same result of creation or annihilation of power as the creation or annihilation (which latter is only the total removal) of either of the acting bodies would do.

Such, I think, must be the character of the conclusion, if it be supposed that the attraction of the sun upon the earth arises *because* of the presence of the earth, and the attraction of the earth upon the sun, because of the presence of the sun : there remains the case of the power, or the efficient source of the power, having pre-existed in the sun (or the earth) *before* the earth (or the sun) was in presence. In the latter view it appears to me that, consistently with the conservation of force, one of three sub-cases must occur : either the gravitating force of the sun, when directed upon the earth, must be removed in an equivalent degree from some other bodies, and when taken off from the earth (by the disappearance of the latter) be disposed of on some other bodies ;—or else it must take up some *new* form of power when it ceases to be gravitation, and consume some other form of power when it is developed as gravitation ;—or else it must be *always* existing around the sun through infinite space. The first sub-case is not imagined by the usual hypothesis of gravitation, and will hardly be supposed probable : for, if it were true, it is scarcely possible that the effects should not have been observed by astronomers, when considering the motions of the planets in different positions with respect to each other and the sun. Moreover, gravitation is not assumed to be a dual power, and in them only as yet have such removals been observed by experiment or conceived by the mind. The second sub-case, or that of a new or another form of power, is also one which has never been imagined by others, in association with

the theory of gravity. I made some endeavours, experimentally, to connect gravity with electricity, having this very object in view (*Phil. Trans.* 1851, p. 1); but the results were entirely negative. The view, if held for a moment, would imply that not merely the sun, but all matter, whatever its state, would have extra powers set up in it, if removed in any degree from gravitation; that the particles of a comet at its perihelion would have changed in character, by the conversion of some portion of their molecular force into the increased amount of gravitating force which they would then exert; and that at its aphelion, this extra gravitating force would have been converted back into some other kind of molecular force, having either the former or a new character: the conversion either way being to a perfectly equivalent degree. One could not even conceive of the diffusion of a cloud of dust, or its concentration into a stone, without supposing something of the same kind to occur; and I suppose that nobody will accept the idea as possible. The third sub-case remains, namely, that the power is always existing around the sun and through infinite space, whether secondary bodies be there to be acted upon by gravitation or not: and not only around the sun, but around every particle of matter which has existence. This case of a constant necessary condition to action in space, when as respects the sun the earth is *not* in place, and of a certain gravitating action as the result of that previous condition when the earth *is* in place, I can conceive, consistently, as I think, with the conservation of force: and I think the case is that which Newton looked at in gravity; is, in philosophical respects, the same as that admitted by all in regard to light, heat, and radiant phenomena; and (in a sense even more general and extensive) is that now driven upon our attention in an especially forcible and instructive manner, by the phenomena of electricity and magnetism, because of their dependence on dual forms of power.

[M. F.]

Friday, January 26.

WILLIAM ROBERT GROVE, Esq. M.A. Q.C. F.R.S.
Vice-President, in the Chair.

PROFESSOR TYNDALL, F.R.S.

*On the Nature of the Force by which Bodies are Repelled from
the Poles of a Magnet.*

THE Lecturer commenced, by showing that bodies are repelled by the poles of a magnet, in virtue of a state of excitement into which they are thrown by the latter. The repulsion of bismuth, and the attraction of soft iron, followed precisely the same laws when the strength of the influencing magnet was augmented, the respective forces being proportional, not simply to the strength, but within wide limits, to the square of the strength of the magnet. The result is explained in the case of iron by the fact of its being converted, while under magnetic influence, into a true temporary magnet, whose power varies with that of the influencing one; and in the case of bismuth, the result can only be explained by the fact that the dia-magnetic mass is converted into a true *dia-magnet*.

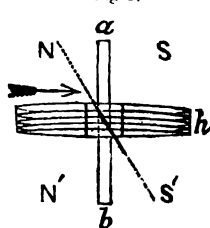
It was next shown that the condition of excitement evoked by a magnetic pole was not the same as that evoked by another pole of an opposite quality. If the repulsion were independent of the quality of the pole, then two poles of unlike names ought to repel the bismuth, when brought to act upon it simultaneously. This is not the case. Two poles of the same name produce repulsion; but when they are of equal powers and opposite names, the condition excited by one of them is neutralized by the other, and no repulsion follows.

Bars of magnetic and dia-magnetic bodies were next submitted to all the forces capable of acting upon them magnetically; first, to the magnet alone; secondly, to the electric current alone; and, thirdly, to the magnet and current combined. Attention to structure was here found very necessary, and the neglect of it appears to have introduced much error into this portion of science. Powdered bismuth, without the admixture of any foreign ingredient, was placed in a strong metallic mould, and submitted to the action of a hydraulic press; perfectly compact metallic masses were thus procured, which, suspended in the magnetic field with the line of compression horizontal, behaved exactly like magnetic bodies, setting their longest dimensions from pole to pole. This identity of deportment with an ordinary magnetic substance was also exhibited

in the case of the electric current, and of the current and the magnet combined. In like manner, by the compression of a magnetic powder magnetic bars were produced, which, between the two poles of a magnet, set exactly like ordinary dia-magnetic ones; this identity of deportment is preserved when the bars are submitted to the action of the current, and of the current and magnet combined. Calling those bars which show the ordinary magnetic and dia-magnetic action *normal bars*, and calling the compressed bars *abnormal ones*, the law follows, that an abnormal bar of one class of bodies exhibits precisely the same deportment, in all cases, as the normal bar of the other class; but when we compare normal bars of both classes together, or abnormal bars of both classes, then the antithesis of action is perfect. The experiments prove that, if that which Gauss calls the *ideal distribution* of magnetism in magnetic bars be inverted, we have a distribution which will produce all the phenomena of dia-magnetic ones.

The important question of dia-magnetic polarity was submitted to further and stricter examination. A flat helix, whose length was an inch, internal diameter an inch, and external diameter seven inches, was attached firmly to a table, with its coils vertical. A suspension was arranged by means of which a bar of bismuth, five inches long, and 0.4 of an inch in diameter, was permitted to swing freely, while surrounded by the helix. With this arrangement, the following experiments were, or might be made:—1. A voltaic current from twenty of Grove's cells was sent through the helix *h*, the direction of the current in the upper half of the helix being that denoted by the arrow (Fig. 1). The north pole of a magnet being placed at N, the end *a* of the suspended bar of bismuth, *a b*, was *attracted* towards the pole N. 2. The south pole of a second magnet being placed at S, and the current being sent through the helix in the same direction as before, the bar left its central position and approached N with greater force than in the former experiment. The reason was deemed manifest: the state of excitement which causes *a* to be attracted by N causes it to be repelled by S; both poles, therefore, act in unison, and a deflection of greater energy is produced. 3. The pole S being removed to the position S', the deflection was also found to be about twice as forcible as when the single pole N was employed. Here also the reason is plain: the two ends, *a* and *b*, of the bismuth bar, are in different states of excitement; the end *a* is attracted by a north pole, the end *b* is attracted by a south pole: both poles act therefore as a mechanical couple upon the bar, and produce the deflection observed. 4. The pole S' was replaced by a north pole of the same strength, thus bringing two poles of the same name to bear

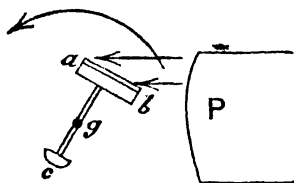
Fig 1.



upon the two ends of the bar : there was no deflection by this arrangement ; it is manifest that N's attraction for the end *a* was nullified by the repulsion of the end *b* by a like pole ; the experiment thus furnishes an additional proof of the polar condition of *a b*. 5. We have supposed the pole S to be removed into the position S' ; but permitting the pole S to remain, and introducing another pole (a south one) at S', a greater action than that produced with two magnets was obtained. 6. Finally, adding another north pole at N', and allowing four magnets to operate upon the bismuth bar simultaneously, a maximum action was obtained, and the bar was attracted and repelled with the greatest promptness and decision. *In all these cases where an iron bar was substituted for the bismuth bar, a b, a deflection precisely the opposite to that exhibited by a b was produced.* A branch of the current by which the bar of bismuth was surrounded could be suffered to circulate round a bar of iron, suspended freely in an adjacent helix ; when the forces acting upon the iron were the same as those acting upon the bismuth, the bars were always deflected in opposite directions.

The question of dia-magnetic polarity was next submitted to a test which brought it under the dominion of the principles of mechanics. A mass of iron was chosen for the moveable magnetic pole, of such a shape that the diminution of the force emanating from the pole, as the distance was augmented, was very slow ; or in other words, the field of force was very uniform. Let the space in front of the pole P, (Fig. 2) be such a field. A *normal* bar of bismuth, *a b*, was attached to the end of a lever transverse to the length of the latter, and counterpoised by a weight at the other extremity : the system was then suspended from its centre of gravity *g*, so that the beam and bar swung horizontally. Supposing the bar to occupy the position shown in the figure, then if the force acting upon it be *purely repulsive*—that is to say, if the dia-magnetic force be unpolar—it is evident that the tendency of the force acting upon every particle of the mass of bismuth tends to turn the lever round its axis of suspension, in the direction of the curved arrow. On exciting the magnetism of P, however, a precisely contrary motion is observed—the lever approaches the pole. This result, which, as far as the lecturer could see, was perfectly inexplicable on the assumption that the dia-magnetic force was purely repulsive, is explained in a simple and beautiful manner on the hypothesis of dia-magnetic polarity. According to this, the end *b* of the bar of bismuth is repelled by P, and the end *a* is attracted : but the force acting upon *a* is applied at a greater distance from the axis of suspension than that acting upon *b* ; and as it has been arranged that the absolute in-

Fig. 2.



tensities of the forces acting upon the two ends differ very slightly from each other, the mechanical advantage possessed by *a* gives to it the greatest moment of rotation, and the bar is attracted instead of repelled. Let a magnetic needle *n s* (Fig. 3,) be attached like the bar *a b* (Fig. 2) to a lever, and submitted to the earth's magnetism. Let the north pole of the earth be towards *N*; the action of the pole upon *n* is attractive, upon *s* repulsive, the absolute intensities of these forces are the same, inasmuch as the length of the needle is a vanishing quantity in comparison with its distance from the pole *N*: hence the mechanical advantage possessed by the force acting upon *s*, on account of its greater distance from the axis of rotation, causes the lever to recede from *N*, and we obtain a result perfectly analogous to that obtained with the bar of bismuth (Fig. 2).*

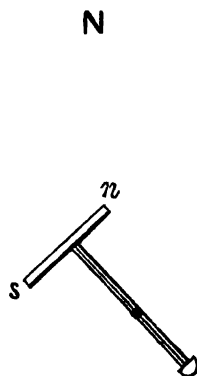


Fig. 3.

[J. T.]

* A paper submitted to the Royal Society last November, and a portion of which formed the subject of the Bakerian Lecture for the present year, contains a more comprehensive discussion of this subject. In it are explanations, which it is hoped will be deemed satisfactory, of the difficulties adduced by M. Matteucci, in his instructive *Cours Special*, recently published.

Friday, March 16, 1855.

SIR HENRY HOLLAND, Bart. F.R.S. Vice-President,
in the Chair.

DR. WM. ODLING, F.C.S.

On the Constitution of the Hydro-carbons.

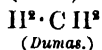
EVERY chemical compound may be regarded in a great number of different aspects. Each of the different theories that have been propounded concerning the chemical constitution of bodies, is true in reference to one particular aspect,—untrue in reference to all others. Theories are of the highest service when they enable us to look upon a larger number of bodies from a single point of view,—of the highest detriment, when they prevent us from making use of all other points of view. To regard a body, or a class of bodies, exclusively in one aspect, or, in other words, to view all compounds by the light of a single theory, is necessarily to neglect a whole host of phenomena and relations. He has the most complete knowledge of a compound, who is capable of changing his position, and looking at the body from every possible point of view.

The theory of compound radicals is of the utmost service in enabling us to look upon a large class of bodies in one single aspect, in affording us one of the best means of arrangement, comparison, and explanation: but it has no pretensions whatever to represent the entire and absolute truth with regard to the constitution of bodies; it simply exhibits them from one of many excellent points of view; it has reference less to the actual constitution of the bodies, than to our particular mode of regarding them.

In proportion to the complexity of the constitution of a body, so is the number of aspects in which it may be regarded, so is the number of rational theories that may be entertained concerning it. All of these theories belong to the same order of truth: they differ from one another only in their greater or less degree of generality. The theory of the greatest generality most nearly approximates to the representation of bodies, especially typical bodies, by empirical formulæ, as unitary molecules.

Adopting the proportional numbers of Gerhardt, we represent the two-volume molecules of muriatic acid, water, ammonia, and coal-gas, by ClH , OH^2 , NH^3 , CH^4 respectively. In accordance

with certain theoretical notions, these bodies have been formulated as follows :—



Coal-gas may be represented as terhydride of formyl, analogous to its derivative chloroform, or terchloride of formyl. The two bodies can be prepared in virtue of analogous equations from acetic and chlor-acetic acids respectively, and the one can be obtained from the other by direct substitution.

Each of the above theories has certain circumstances in its favour ; each is true to a certain extent ; each represents the body in question from a different point of view ; sometimes one point of view is most advantageous, sometimes another. As a veritable representation of the constitution of coal-gas, Dumas' view is preferable to either of the other two theoretical views.

The adoption of Laurent's sarcastic suggestion of peroxide of hydrogen as a compound radical, leads to inadmissible or uncertain results ; thus, is potash oxide of zinc KZ O a combination of a hypothetical peroxide of potassium with zinc, or of a hypothetical peroxide of zinc with potassium ? Is Williamson's double ether, Me Et O , a combination of peroxide of methyl with ethyl, or of peroxide of ethyl with methyl ? &c.

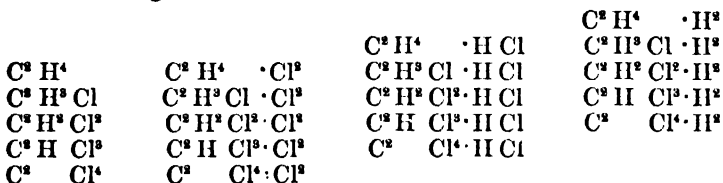
Nevertheless, there are greater grounds for recognising peroxide of hydrogen as a compound radical, than there are for recognising ethyl and methyl as such. A large number of bodies may be represented very feasibly as containing ethyl ; but an infinitely larger and more varied set of bodies may be represented as containing peroxide of hydrogen : such, for instance, are water, potash, sulphuric acid, formic acid, benzoic acid, hypochlorous acid, &c. &c. and, as has been shown by Mr. Brodie, very many other more complicated bodies. Many equations may be represented very simply by means of ethyl analogous to hydrogen ; but a much greater number may be represented by means of peroxide of hydrogen analogous to chlorine. Ethyl has been obtained in the free state, so has peroxide of hydrogen ; but whereas nearly all the bodies of the peroxide of hydrogen series can be obtained directly from it, not one single ethylic combination has ever yet been obtained from ethyl. Hydrogen and ethyl present certain analogies, but the analogies of chlorine and peroxide of hydrogen are much more complete. Both bodies bleach, oxidise, combine directly with potassium, set free bromine and iodine, and take the place of the bromine and iodine set free. In $\text{Ba} \cdot \text{OH}$ and in Ba Cl the Ba

can be readily detected; but with regard to H Cl and Et Cl, the Cl can be detected in the former only.

All the facts connected with the mutual relations of

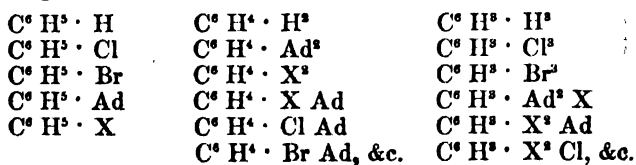
$C^2 H^4$ —ethylene, or olefiant gas
 $C^2 H^4 O$ —aldehyd
 $C^2 H^4 O^2$ —acetic acid
 $C^2 H^6$ —hydro-ethylene
 $C^2 H^3 Cl$ —muriatic ether
 $C^2 H^6 O$ —alcohol,

especially since the recent researches of Berthelot, show the superiority of Dumas' ethylene to Liebig's ethyl theory, both as regards its more complete accordance with experiment, and its greater generality. The probabilities in favour of the pre-existence of $C^2 H^4$ and its derivatives, as constituent groups, are much greater than are those in favour of the pre-existence of $C^2 H^2$. Thus, with regard to ethylene, hydro-ethylene, muriatic ether, and their chlorine derivatives, we ought to have the following series, convertible into one another through certain members:—



Of these four series, three are undoubtedly, and the fourth most probably, known to us. They illustrate rationally the nature of the isomerism. In the three latter series, we have every reason to believe, that, with regard to the carbon, two of the hydrogen or chlorine atoms stand in a different relation to the other four; but in the first series, we have not a single fact tending to show, that one of the hydrogen atoms stands in a different relation to the other three; not one fact to countenance the representation of olefiant-gas by $C^2 H^2 \cdot H$, hydruret of acetyl.

In the next best known hydro-carbon, namely, benzine, there is no more reason for believing in the existence of the monobasic radical phenyl $C^6 H^5$, than there is for believing in the bibasic and tribasic radicals $C^6 H^4$ and $C^6 H^3$, respectively, as seen in the following table:—($X = NO^2$ Ad = NH^2)



Lastly, Williamson's othyl theory, although it possesses a great degree of generality, and is supported by most complete analogies both in mineral and in organic chemistry, is only one of many ways of indicating the mutual relations of bodies. It must not be taken as the sole veritable representation of the constitution of the compounds to which it applies. There are no greater proofs of the pre-existence of othyl in acetic acid, than there are of the pre-existence of peroxide of hydrogen in water.

For example, the correlations of benzamide, benzonitril, hydrobenzamide, and dibenzoylimide, are entirely neglected in the othyl theory. These bodies belong to one single class; they all contain certain benzoic elements, and certain ammoniacal elements; by the absorption of water they yield ammonia, and benzoic acid or aldehyd. But the othyl theory bears no reference to this point of view; it separates benzamide widely from its congeners. Thus we are told that the first body contains the compound radicals benzoyl (analogous to othyl) and amidogen; the second, the compound radicals phenyl and cyanogen; the third, nitrogen and the compound radical benzyl (analogous to acetyl), whilst, with regard to the fourth, as to many other bodies, the compound radical theory fails altogether.

In the three best known hydro-carbons, coal-gas, olefiant-gas, and benzine, as in many other bodies ordinarily represented as containing compound radicals, the conception of self-existent constituent compound radicals, is not only unnecessary but irrational. The particular groupings of atoms, which we denominate compound radicals, do not have an existence apart from the other constituents of the bodies, into which they are said to enter.

[W. O.]

Friday, March 23.

HENRY BENCE JONES, M.D. F.R.S.
in the Chair.

REV. JOHN EYRE ASHBY,

*On (so-called) Catalytic action and Combustion; and
theories of Catalysis.*

THE study of Catalysis is a study of forces. It comprehends the conditions under which force is exerted in a peculiar manner in chemical decompositions and combinations, and of the nature of the force only as compared with forces at work in chemical changes, which are commonly thought to be better understood. The precise meaning of the word should not be derived from its etymological constitution, nor even solely from its original application, but rather from the general consent of eminent chemists as exhibited in their published writings. The term has been employed very loosely, and some of the definitions given do not allow of reduction to a common statement. Adopting the principle laid down, catalysis may be defined as the action by contact of one substance upon another substance, or group of substances, whereby chemical changes are effected, while the first substance remains finally unchanged. This will *exclude* from catalysis the following cases:—

1. The operating body does change, although it does not combine.

Examples.—Fermentation, as explained by Liebig. Solution of an alloy of platinum and silver in nitric acid; also of the alloy of copper, zinc, and nickel, in dilute sulphuric acid.

2. The operating body absorbs A from a combination ($A + B$), and B cannot exist free.

Example.—Crystallized oxalic acid is a combination of anhydrous oxalic acid and water; if cast into strong sulphuric acid, the water is absorbed, and (so far as we know at present) taken into chemical union with the sulphuric acid; but the anhydrous oxalic acid cannot exist free, and is resolved into equal volumes of carbonic acid and carbonic oxide, which escape accordingly.

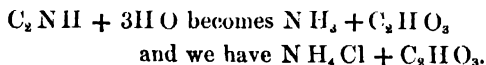
3. The operating body takes from a substance some of its elements, and combines with them after they have combined with each other.

Example.—Sulphuric acid, in contact with sugar, takes the

elements of water from the carbon, and combines with them by hydration.

4. The operating body causes mutual changes in substances A and B, whereby they become other substances, say C and D, and the operating body combines with D.

Example.—Hydrocyanic acid and water + hydrochloric acid (the operating body) gives formic acid and ammonia, of which the ammonia combines with the hydrochloric acid.



Cases of chemical change by friction and percussion are excluded, because *contact* is mechanically statical, whereas these are dynamical in respect of the masses. A lucifer match is an example. Sir James Kane, however, supposes the decomposition of iodide of nitrogen by slight percussion to be catalytic; his definition is made wide enough to take it in.

The definition given by the speaker as being, upon the whole, in harmony with the prevailing ideas of catalysis, will be found to include the following cases:—

1. Galvanic action in relation to a finally unchanged platinode. In every battery-cell containing an apparently unchangeable platinode, all the conditions of catalysis are perfectly fulfilled when the platinode touches the zincode without the intervention of any other metallic conductor; and the truthfulness of the fulfilment is not altered by the additional fact that similar action will ensue when a metallic (or other) conductor is interposed. It is very doubtful whether the platinode really undergoes no changes, but it remains unchanged at last.

2. A finally unchanged body produces in certain others no decomposition, but only combination.

Example.—Platinum combines hydrogen and oxygen into water.

3. A finally unchanged body produces decomposition, but no re-combination.

Example.—Decomposition of chlorate of potash by metallic oxides and heat.

4. A finally unchanged body produces decomposition, and partial or total recombination, of the elements of a body.

Example.—Alcohol passed into sulphuric acid at 300° passes out as ether and water, the elements of water having been abstracted by the acid, which cannot retain them at that temperature. If we suppose that the acid removes water, *as such*, from the alcohol, this case is resolved into the foregoing.

5. A finally unchanged body produces the decomposition of

another substance, and causes some of its elements to enter into combination with a third body, also present.

Examples.—Heated platinum decomposes alcohol vapour, with access of air. In illustration of this class of phenomena, the speaker exhibited the continuous combustion of the vapour of strong liquid ammonia by a flat spiral of platinum wire ($\frac{1}{10}$ inch thick) evolving nitrous gas and water.

Mr. Ashby then briefly adverted to the principal theories of Catalysis.

1. Berzelius, who introduced the term into chemistry, considers that it represents a new force.

2. Liebig dwells at great length upon cases in which the operating body does *not* remain finally unchanged. He supposes that if A and B are in contact, and changes happen among the particles of A, these may induce changes among the particles of B, by destroying the statical condition, and forcing the particles into motion, whereupon they arrange themselves into new groups. This does not explain at all the cases included under the definition given in this discourse. It is just possible that it may ultimately prove to be a truth in relation to those cases, but it does not, *at present*, explain them.

3. Playfair observes, "There are many instances where catalytic decompositions ensue, where there is no intestine motion in the atoms of the exciting body. Hence we cannot do more than consider motion as favourable to the development of dormant affinities." He should, perhaps, have said, "Where there is no *apparent* intestine motion;" and there may be an undesirable ambiguity in the phrase, "favourable to the development of dormant affinities." In his essay, on the subject, (a monument of learning and industry,) he concludes, that "the catalytic body is a substance which acts by adding its own affinity to that of another body, or by exerting an attraction sufficient to effect decomposition under certain circumstances, without being powerful enough to overcome new conditions, such as elasticity and cohesion, which occasionally intervene, and alter the expected result." (Thus, for instance, A and B have each separately an affinity for the element (*e*) of a third body E, but neither, separately, can tear it from its combination with E:—the joint attraction of A and B *may* be sufficient to release it; but the released element will not therefore necessarily combine with A and B.)

4. Faraday, and others, consider that many cases of catalysis are due to the powerful condensation of liquids, vapours, and gases, upon the surface of the catalyzing body. This view is of great importance, but not (as yet) equal to the explanation of nearly all the phenomena. Some of the arguments in favour of it may be found in a consideration of the probable mechanical condition of platinum, owing to the method of manufacture, the ascertained

absorptive power of certain catalyzing agents, and the probable condensation of water in sulphuric acid.

5. De la Rive explains the particular case of the combination of hydrogen and oxygen on the surface of platinum, by supposing that the platinum is oxidated on its surface, and the oxide continuously reduced by the hydrogen, water resulting. (See Gmelin's Chemistry, published by the Cavendish Society, Vol. II. p. 56.)

Mr. Ashby then described his own researches on catalytic combustion. If the sesquioxides of chromium, nickel, cobalt, manganese, and iron, be laid upon wire-gauze (about 60 meshes to the inch-linear,) then warmed in the flame of a spirit-lamp, and laid over capsules nearly full of alcohol, pyroxylic spirit, ether, or other similar substances, they will burst into glow and catalyze the vapours which rise into them, as long as the supply continues. Warm Cr_2O_3 will inflame a jet of hydrogen in contact with air. The sesquioxide of iron is peculiarly available for operations on any scale, however large, and those specimens are to be preferred which have the least density. A catalytic lamp was exhibited, by which spirit or benzole may be consumed by the catalytic glow for laboratory purposes. Coal-gas mixed with air may also be employed under the gauze upon which the oxide is distributed. Euchrome (dug from the estate of Lord Audley) is a cheap and useful catalyzing agent, when freed from its carbon.* A mixture of ten parts by weight of chlorate of potash, with one part of light and finely divided sesquioxide of iron, yields oxygen with entireness and facility, and has the additional advantage that (n) grains of the mixture represent very nearly (n) cubic inches of evolved oxygen. If we take the case of the sesquioxide of iron, we are able, *to some extent*, to show the *modus operandi* of the catalytic action; we can arrest the process at the half-stage, and then at leisure complete the other half. By heating Fe_2O_3 to redness, and quenching it in boiling alcohol (air being excluded), or by passing the vapour of alcohol over heated sesquioxide, we obtain two results; the alcohol is oxidated, and the Fe_2O_3 becomes Fe_3O_4 by deoxidation. The same follows by treating the sesquioxide with ammonia, gaseous or in strong solution. This black magnetic oxide (probably $\text{Fe}_3\text{O}_4 + \text{H}_2\text{O}$) remains unchanged by a red heat, if oxygen be not present; but if warmed in contact with air, it absorbs oxygen at a temperature far below redness, and returns into the original Fe_2O_3 . It is clear, then, that during the catalysis, an intestine motion of the particles is going forward, and every portion of the sesquioxide is constantly reduced by the alcohol and reoxidated from the air. This was illustrated by a diagram and a cluster of coloured balls. Attention was

* Mr. Arthur Church has observed that several of the chromates (copper, manganese, cobalt, &c.) after ignition, will freely catalyze alcoholic and other vapours.

then drawn to a singular fact, which may probably be referable to the catalytic action of the sesquioxide of iron. If the ferrocyanide of barium be heated until it ceases to give off ammonia, and then placed over alcohol, ammonia is again evolved in small quantity; but if the same experiment be tried with pure prussian blue, a large quantity of ammonia is formed by the contact with alcoholic vapour. Alumina (Al_2O_3) presents a singular phenomenon, hitherto unobserved; a pure specimen, of snowy whiteness, on being heated to redness, and exposed to the vapour of alcohol, becomes dark-grey, or black, and the vapour is oxidated; and this effect may be produced (with less ease) by ammonia. It seems that the sesquioxide of alumina has become a lower oxide, as in the case of iron, but cannot recover oxygen from contact with that gas.

Mr. Ashby then exhibited a "galvanic indicator," by which he hopes to prove satisfactorily, whether or not a galvanic current is set in motion during catalytic combustion. In conclusion, he drew attention to several points of interest connected with catalysis.

1. The value of catalytic processes in various manufactures. Nitric oxide acts catalytically in the preparation of sulphuric acid of commerce; spongy platinum has been employed for the same purpose; and a patent has lately been granted for a similar use of the sesquioxide of iron. Spongy platinum is sometimes employed in Germany for the preparation of acetic acid from alcohol. The manufacture of ether from alcohol by sulphuric acid has already been noticed.

2. Considering that catalytic combustion by the sesquioxide of iron is first set up at a temperature far below redness,—that even a rusted nail may be sufficiently active,—and that in many cases of fermentation inflammable vapours may be disengaged, and much heat evolved,—it is not unreasonable to suppose that in this way we may account for some instances of what is commonly called "spontaneous combustion."

3. There is much reason to suppose that catalysis plays a great part in the organic chemistry of nature, in relation to vegetable and animal life. The experiments of Mr. Turnbull and Dr. Stenhouse point to catalysis as a vast sanitary agent. The dead bodies of various animals were covered with a layer of charcoal, which rather assisted decomposition than otherwise, serving as a carrier to the oxygen of the atmosphere, and delivering the perfectly innoxious and inodorous products of catalytic combustion into the air, while the charcoal itself remained entire and unconsumed. If we bear in mind (as established by Mulder and others) that humus has the same property, and that many oxides in the mould are catalytic in a greater or less degree, it becomes evident that interment underground may bring into play a catalytic process by which the

elements of an organic body are returned into the atmosphere in forms which are not prejudicial to existing life.

4. Since some oxides exhibit a tendency to promote catalytic combustion, which are at present thought to be the only oxides of those bases, and in the case of one oxide (iron) the process has been shown to consist of alternate reduction to a lower oxide, and re-oxidation,—it is quite possible that further research may lead to the discovery of some new oxides.

[J. E. A.]

Friday, May 25.

SIR CHARLES FELLOWS, Vice-President, in the Chair.

PROFESSOR FARADAY, D.C.L. F.R.S.

On Electric Conduction.

SINCE the time when the law of definite electrolytic action was first laid down (*Exp. Res.* 783-966), it has become a question whether those bodies which form the class of electrolytes, conduct only whilst they are undergoing their proper change under the action of the electric current; or whether they can conduct also as metals, dry wood, spermaceti, &c. do in different degrees, *i. e.* without the accompaniment of any chemical change within them. The first kind of conduction is distinguished as the *electrolytic*; the transference of the electric force appearing to be essentially associated with the chemical changes which occur: the second kind may be called *conduction proper*; and there the act of conduction leaves the body ultimately as it found it. Electrolytic conduction is closely associated with the liquid state, and with the compound nature and chemical proportions of the bodies in which it occurs; and it is considered as varying in degree (*i. e.* in facility) with the affinities of the constituents belonging to these bodies; there are, however, other circumstances which evidently, and indeed very strongly affect the readiness of transfer, such as temperature, the presence of extraneous matters, &c. Conduction proper differs as to facility by degrees so far apart, that that quantity of electricity which could pass through a hundred miles of one substance, as copper, in an inappreciably small portion of time, would require ages to be transmitted through the like length of another substance, as shell-lac; and yet the copper with its similars offers resistance to conduction; and the lac, and its congeners, conduct.

The progress and necessities of science have rendered it important within the last three or four years, and especially at the present moment, that the question "whether an electrolyte has any degree of conduction proper" should be closely considered, and the experiments which are fitted to probe the question have been carried to a very

high degree of refinement. Buff,* by employing the electric machine, and Wollaston terminals, *i.e.* platinum wires sealed into glass tubes, and having the ends only exposed, has decomposed water by a quantity of electricity so small that it required four hours to collect gas enough to fill a little cylinder only $\frac{1}{10}$ th of an inch in diameter, and the $\frac{1}{2}$ th of an inch in length; yet the decomposition was electrolytic and polar; and therefore the conduction was electrolytic also. When one pole only was in the water, and the other in the air over it, still the decomposition, and therefore the conduction, was electrolytic; for one element appeared at the pole in the water, and the other in the air or gas over the water at the corresponding pole. Buff concludes that electrolytes have no conduction proper. Many other philosophers have supported, with more or less conviction, the same view, and believe that electrolytic conduction extends to, and includes cases, which formerly were supposed to depend upon conduction proper. Soret advances certain experimental results,† but reserves his opinion from being absolute. Von Breda and Logeman adopt the more general view unreservedly.‡ De la Rive, I think, admits that a very little may perhaps pass by conduction proper, but that electrolytic conduction is the function of electrolytes.§ Matteucci has at one time admitted a little conduction proper, but at present, I believe, denies that any degree exists. On the other hand, Despretz,|| Leon Foucault,¶ Masson,** and myself, have always admitted the possibility that electrolytes possess a certain amount of conduction proper—small indeed, but not so small as to prevent its being evident in certain forms of experiments: and beautiful and close as the electrolytic proofs have been carried, they are not by us considered as sufficient to show that the function of conduction proper is altogether absent from electrolytes.

(Some account was then given of the experiments and arguments on both sides; and of the striking electrolytic fact, that if a current of electricity, however small, is sent through a circuit containing a couple of platina plates in dilute sulphuric acid, the plates are found thereby electrically polarized.)

The enquiry as regards electrolytes takes on three forms. They may possess a degree of conduction proper at all times—or they may be absolutely destitute of conduction proper—or they may possess conduction proper up to a certain condition, governed either by requisite intensity for electrolyzation or by other circumstances, but which, when that condition is acquired, changes into electrolytic conduction; and these three forms may be further varied by considerations dependent upon the physical state of the electrolyte, as whether it be solid or liquid, hot or cold, and whether it be pure or contain other substances mingled with it.

* MS. letter. † *Annales de Chimie*, xlii. 257. ‡ *Phil. Mag.* viii. 465.

§ *Bibl. de Genève*, xxvi. 134, 144; xxvii. 177. || *Comptes Rendus*, xxxviii. 897.

¶ *Comptes R.*, xxxvii. 580; or *Bibl. de Genève*, xxiv. 263; xxv. 180; xxvi. 126.

** *Prize Essay, Haarlem Trans.*, xi. 78.

From the time when the question was raised by myself, twenty years ago, to the present day, I have found it necessary to suspend my conclusions; for close as the facts have in certain cases been urged by those who believe they have always obtained decomposition results, when an electrolyte has performed the part of a conductor, and freely as I could have admitted the facts and the conclusions if there had been no opposing considerations, still, because there are such considerations, I am obliged to reserve my judgment. In the first place all bodies not electrolytic, even up to gases (Becquerel,) are admitted to possess conduction proper; *a priori*, therefore, we have reason to expect that electrolytes will possess it also. If from amongst different bodies we retain for consideration the class of electrolytes only, then though the amount of electricity of a given intensity which these can transmit electrolytically when they are *fluid*, is often almost infinitely greater than that which they can convey onwards by conduction proper, when they are *solid*; still the conduction in the latter cases is very evident. A piece of perfectly dry solid nitre, and of many other electrolytes, discharges a gold leaf electrometer very freely, and I believe by the power of conduction proper; and that being the case, I do not see that the assumption of the very highest condition of electrolytic conduction when the nitre is rendered fluid is any argument for the absolute disappearance of the conduction proper which belonged to the body in the solid state, though it may override the latter for the time and make it insensible. These considerations are, however, such as arise rather from the absence of the final and strict proof on the opposite side, than from any thing very positive in their own character; but it has occurred to me that the phenomena of static electricity will furnish us with many reasons of a positive nature, in favour of the possession by liquid electrolytes of the power of conduction proper. Some of these I will endeavour briefly to state, illustrating the subject by a reference to water, which in its pure state has but a low degree of electrolytic conduction.

The ordinary phenomena of static charge and induction are well known. If an excited glass rod or other body be held near a light gilt sphere, suspended from the hand by a metal thread, the inductive action disturbs the disposition of the electricity in the sphere, and the latter is strongly attracted: if in place of the sphere a soap bubble be employed, the same results occur. If a dish filled with pure distilled water be connected with the earth by a piece of moist bibulous paper, and a ball of excited shell-lac be suspended two or three inches above the middle of the water,—and if a plate of dry insulating gutta-percha, about eight inches long and two inches wide, have its end interposed between the water and the shell-lac, it may then be withdrawn and examined, and will be found without charge, even though it may have touched the shell-lac; but if the end once touch the water under the lac (and it

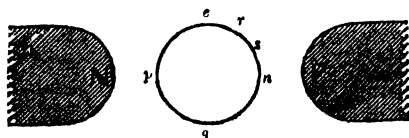
may be dipped in,) so as to bring away a film of it, charged with the electricity the water has acquired by the induction, it will be found to possess, as might be expected, a state contrary to that of the inductric shell-lac.

In order to exclude any conducting body but water from what may be considered as a reference experiment, two calico globular bags with close seams were prepared; and being wetted thoroughly with distilled water, were then filled with air by means of a fine blow-pipe point; they were then attached to two suspending bands of gutta-percha, by which they were well insulated, and being about three inches diameter they formed, when placed in contact, a double system six inches in length. A metallic ball, about four inches in diameter, was connected with the electric machine to form an inductric body, an uninsulated brass plate was placed about nine inches off to form an inductive body; between these the associated water balls could be placed so as to take part in the induction, and when the electric charge was so low that the moist atmosphere caused no transmission of electricity, the balls could be introduced into position and brought away without having received any permanent charge. Under these circumstances if the associated balls were brought into the place of induction, were then separated, withdrawn, and examined, they were found, the one charged positively and the other negatively, by electricity derived from themselves, and without conductive or convective communication with any other substance than their own water.

It is well known indeed that by the use of water we may replace metal in all electro-static arrangements, and so form Leyden jars, condensers, and other induction apparatus, which are perfect in principle though with imperfect action. The principles are the same, whether water or metals be used for conductors, and the function of conduction is essential to all the results; therefore conduction cannot be denied to the fluid water, which in all such cases is acting as the only conductor. In nature, indeed, the phenomena of induction, rising up to their most intense degree in the thunder-storm, are almost, if not altogether, dependent upon the water which in the earth, or the clouds, or the rain, is then acting by its conducting power; and if this conducting power be of the nature of *conduction proper*, it is probable that that function is as large and as important as any exercise of the electrolytic conduction of water in other natural phenomena.

But it may be said that all these cases, when accompanied by conduction, involve a corresponding and proportionate electrolytic effect, and are therefore cases of electrolytic conduction; and it is the following out of such a thought that makes me think the results prove a conduction proper to exist in the water. For suppose a water bubble to be placed midway between a positive and a negative surface, as in the figure, then the parts at and about p will become charged positive, and those at and about n negative, solely

by the disturbance of the electric force originally in the bubble, *i. e.* without any direct transmission of the electric force from *N* or *P*; the parts at *e* or *q* will have no electric charge, and from those parts to *p* and *n* the charge will rise gradually to a maximum. The electricity which appears at *p*, *n*, and elsewhere,



will have been *conducted* to these parts from other parts of the bubble; and if the bubble be replaced by two hemispheres of metal, slightly separated at the equatorial parts *e q*, the electricity (before *conducted* in the continuous bubble,) will then be seen to pass as a bright spark. Now the particles at any part of the water bubble may be considered under two points of view, either as having had a current passed through them, or as having received a charge; in either view the idea of conduction proper supplies sufficient and satisfactory reasons for the results; but the idea of electrolytic conduction seems to me at present beset with difficulties. For consider the particles about the equator *e q*, they acquire no final charge, and they *have conducted*, as the action of the two half spheres above referred to show; and they are not in a state of mutual tension, as is fully proved by very simple experiments with the half hemispheres. Therefore oxygen must have passed from *e* towards *n*, hydrogen from *e* towards *p*, *i. e.* towards and to the parts to which the electricity has been conducted, for without such transmission of the anions and cations there would be no transmission of the electricity, and so no electrolytic conduction. But then the questions arise,—Where do these elements appear? is the water at *n* oxygenated, and that about *p* hydrogenated? and may the elements be at last dispersed into the air at these two points, as in the case of decompositions against air poles? (*Exp. Res.* 455, 461, &c.) In regard to such questions other considerations occur respecting the particles about *p* and *n*, and the condition of charge they have acquired. These have received the electricity which has passed as a current through the equatorial parts, but they have had no current or no proportional current through themselves—the conduction has extended to them but not *through* them; no electricity has passed for instance through the particle at *n* or at *p*, yet more electricity has gone by some kind of conduction to them than to any other of the particles in the sphere. It is not consistent with our understanding of electrolytic conduction to suppose that these particles have been charged by such conduction; for in the exercise of that function it is just as essential that the electricity should *leave* the

decomposing particle on the one side, as that it should *go to it* on the other : the mere escape of oxygen and hydrogen into the air is not enough to account for the result, for such escape may be freely permitted in the case of electrodes plunged into water ; and yet if the electricity cannot pass from the decomposing particles into the electrodes, and so away by the wires, in a condition enabling it to perform its full equivalent of electric work any where else in the circuit, there is no decomposition at the final particles of the electrolyte, nor any electrolytic conduction in its mass. Even in the air cases above referred to there is a complete transmission of the electricity across the extreme particles concerned in the electrolysis.

If the above reasoning involve no error, but be considered sufficient to show that the particles at *p* and *n* are not electrolyzed, then it is also sufficient to prove that none of the particles between *p* and *n* have been electrolyzed ; for though one at *e* or *q* may have had a current of electricity passed through it, it could not give up its elements unless the neighbouring particles were prepared to take them in a fully equivalent degree. To stop the electrolysis at *n* and *p*, or at those parts of the surface where the moving electricity stops, is to stop it at all the intervening parts according to our present views of electrolysis, and to stop the electrolysis is to shut out electrolytic conduction ; and nothing at present remains but *conduction proper*, to account for the very manifest effects of conduction which occur in the case.

It may be imagined that a certain polarized state of tension occurs in these cases of static induction, which is intermediate between it and electrolytic conduction (*Exp. Res.* 1164) ; or that a certain preparatory and as it were incomplete condition may be assumed, distinguishing the case of static conduction with globes of water, which I have taken as the ground of consideration from the same case when presented by globes of metal. Our further and future knowledge may show some such state ; but in respect of our present distinctive views of conduction proper and electrolytic conduction, it may be remarked that such discovery is just as likely to coincide with the former as with the latter view, though it most probably would alter and correct both.

Falling back upon the consideration of the particles between *e* and *n*, we find, that whether we consider them as respects the current which has passed through them, or the charge which they have taken, they form a continuous series ; the particle at *e* has had most current, that at *n* none, that at *r* a moderate current ; and there are particles which must have transmitted every intermediate degree. So with regard to charge ; it is highest at *n*, nothing at *e*, and every intermediate degree occurs between the two. Then with respect to these superficial particles, they hold all the charge that exists, and therefore all the electricity which has been conducted is in them ; consequently all the electrolytic results must be there ; and that would be the case, even though for the shell we

were to substitute a sphere of water. For, if those particles which have had more current through them than others be supposed to have more of the electrolytic results about them than the others, then that electricity which is found associated chiefly, if not altogether, with these others, could have reached them only by conduction proper, which for the moment is assumed to be non-existent. So, to favour the electrolytic argument, we will consider the conduction as ending at, and the electrolytic results as summed up in, these superficial particles, passing for the present the former objection that though the electricity has reached, it has not gone through, these particles. Taking, therefore, a particle at r , and considering its electrolytic condition as proportionate to the electricity which has arrived at that particle, and given it charge, we may then assume, for we have the power of diminishing the inductive action in any degree, that the electricity, the conduction of which has ceased upon the particle that was there has been just enough to decompose it, and has left what was the under but is now the surface particle, charged. In that case, some other particle, in a higher state of charge, and nearer to n , as at s , will have had enough electricity conducted towards its place to decompose two particles of water;—but it is manifest that this cannot be the next particle to that at r , but that a great number of other particles in intermediate states of charge must exist between r and s . Now the question is, how can these particles become intermediately charged by virtue of electrolytic conduction only? Electrolytic action is definite, and the very theory of electrolytic conduction assumes that the particles of oxygen and hydrogen as they travel convey not a variable but a perfectly definite amount of power onward in its course, which amount they cannot divide, but must take at once from a like particle, and give at once to another like particle. How then can any number of particles, or any action of such particles carry a fraction of the force associated with each particle? It is no doubt true, that if two charged particles can throw their power either on to one, or to three or more other particles, then all the difficulty disappears. Conduction proper can do this: but, as we cannot conceive of a particle half decomposed, so I cannot see how this can be performed by electrolytic conduction, *i.e.* how the particle between r and s can be excited to the intermediate and indefinite degree, conduction without electrolysis being denied both to it and the particles around it.

If the particles between e and n be supposed to conduct electrolytically by the current which passes *through* them (dismissing for a time, amongst other serious objections, that already given that the products would not be found at the places to which the electricity has been conveyed) still the present argument would have like force. At r enough electricity may have passed through to decompose two particles of water, at s only enough to decompose one,—how is a particle between r and s to change elements with the par-

ticles either towards r or towards s , if electrolytic change only is to be admitted? or how, as before enquired, can two particles throw their power on to, or receive their power from one? Many other considerations spring out of the thought of a water bubble, under static induction; but these just expressed, with those that relate to the *seat* of electrolytic action, whether at the place of current or of charge, create a sum of difficulty fully sufficient, without any others, to make me suspend for the present any conclusions on the matter in question.

The conduction power of water may be considered under another point of view; namely, that which has relation to the absolute charge that can be given to the fluid. A point from the electrical machine can charge neighbouring particles of air, and they issue off in streams. It can do the same to particles of camphine, or oil of turpentine;—it can do the same to the particles of water; and if two fine metallic wires connected with Ruhmkorff's apparatus, be immersed in distilled water, about half an inch apart, the motes usually present will soon show how the water receives charge, and how the charged water passes off in streams, which discharge to each other in the mass. Now such charge is not connected with electrolysis; the condition of electrolyzation is that the electricity pass through the water and do not stop short in it. The mere charge of the water gives us no idea where any constituents set loose by electrolysis can be evolved, and yet conduction is largely concerned in the act of charging. A shower of rain falls across a space in the atmosphere subject to electric action, and each drop becomes charged; spray may be thrown forth from an electrified fountain very highly charged;—conduction has been eminently active in both cases, but I find it very difficult to conceive how that conduction can be electrolytic in its character.

When drops of water, oppositely electrified, are made to approach each other, they act by convection, *i. e.* as carriers of electricity; when they meet they discharge to each other, and the function of conduction is for the time set up. When the water bubble, described p. 5, is taken out of the sphere of induction, the opposite electricities about p and n neutralize each other, being conducted through the particles of the water. Are we to suppose in these cases that the conduction is electrolytic? if so, where are the constituents separated, and where are they to appear? It must be a strong conviction that would deny conduction proper to electrolytes in these cases; and if not denied here, what reason is there ever to deny it absolutely.

The result of all the thought I can give to the subject is a suspended judgment. I cannot say that I think conduction proper is as yet disproved in electrolytes; and yet I cannot say that I know of any case in which a current, however weak, being passed by platinum electrodes across acidulated water does not bring them into a polarized condition. It may be that when metallic surfaces are

present, they complete by their peculiarities the condition necessary to the evolution of elements, which, under the same degree of electrification would not be evolved if the metals were away ; and, on the other hand, it also may be that after the metals are polarized, and a consequent state of reactive tension so set up, a degree of conduction proper may occur between them and the electrolyte simultaneously with the electrolytic action. There is now no doubt that as regards electrolysis and its law, all is as if there were but electrolytic conduction ; but, as regards static phenomena (which are equally important) and the steps of their passage into dynamic effects, it is probable that conduction proper rules with electrolytes as with other compound bodies : for it is not as yet disproved, is supported by strong presumptive evidence, and may be essential. Yet so distant are the extremes of electric intensity, and so infinitely different in an inverse direction are the quantities that may and do produce the essential phenomena of each kind, that this separation of conductive action may well seem perfect and entire to those whose minds are inclined rather to see conduction proper replaced by electrolytic conduction, than to consider it as reduced, but not destroyed ; disappearing, as it were, for electricity of great quantity and small intensity, but still abundantly sufficient for all natural and artificial phenomena, such as those described, where intensity and time both unite in favouring the final results required.

But we must not dogmatise on natural principles, or decide upon their physical nature without proof ; and, indeed, the two modes of electric action, the electrolytic and the static, are so different yet each so important, the one doing all by quantity at very low intensity, the other giving many of its chief results by intensity with scarcely any proportionate quantity, that it would be dangerous to deny too hastily the conduction proper to a few cases in static induction, where water is the conductor, whilst it is known to be essential to the many, only because, when water is the electrolyte employed, electrolytic conduction is essential to every case of electrolytic action.

[M. F.]

Friday, June 1.

THE DUKE OF NORTHUMBERLAND, K.G. F.R.S. President,
in the Chair.

PROFESSOR TYNDALL, F.R.S.

On the Currents of the Leyden Battery.

IN our conceptions and reasonings regarding the forces of nature we perpetually make use of symbols, which, when they possess a high representative value, we dignify with the name of theories. We observe, for example, heat propagating itself through a bar of metal, and help ourselves to a conception of the process by comparing it with water percolating through sand, or travelling by capillary attraction through a lump of sugar. In some such way we arrive at what is called the material theory of heat. The thing seen is thus applied to the interpretation of the thing unseen, and the longing of the human mind to rest upon a satisfactory reason, is in some measure satisfied. So also as regards the subject of the present evening's discourse; we are not content with the mere facts of electricity; we wish to look behind the fact, and prompted by certain analogies we ascribe electrical phenomena to the action of a peculiar fluid. Such conceptions have their advantages and their disadvantages: they afford peaceful lodging to the intellect for a time, but they also circumscribe it; and by and by, when the mind has grown too large for its mansion, it often finds a difficulty in breaking down the walls of what has become its prison instead of its home. Thus, at the present day, the man who would cross the bounds which at present limit our knowledge of electricity and magnetism finds it a work of extreme difficulty to regard facts in their simplicity, or to rid them of those hypothetical adornments with which common consent has long invested them.

But though such is the experience of the earnest student of Natural Philosophy at the present—though he may be compelled to refuse his assent to the prevalent theoretic notions, he may never-

theless advantageously make use of the language of these theories in bringing the facts of a science before a public audience; and in speaking of electricity, the speaker availed himself of the convenient hypothesis of two fluids, without at all professing a belief in their existence. A Leyden jar was charged. The interior of the jar might be figured as covered with a layer of positive electricity, and the exterior by a layer of negative electricity; which two electricities, notwithstanding their mutual attraction, were prevented from rushing together by the glass between them. When the exterior and interior coating are united by a conducting body, the fluids move through the conductor and unite; thus producing what is called an electric current. The mysterious agent which we darkly recognise under this symbol is capable of producing wonderful effects; but one of its most miraculous characteristics is its power of arousing a transitory current in a conductor placed near it. The phenomena of voltaic induction are well known; and it is interesting to inquire whether frictional electricity produces analogous phenomena. This question has been examined by Dr. Henry, and still more recently by that able and experienced electrician M. Riess, of Berlin. The researches of these gentlemen constituted the subject of the evening's discourse.

A wooden cylinder was taken, round which two copper wires, each 75 feet in length, were wound; both wires being placed upon a surface of gutta-percha, and kept perfectly insulated from each other. The ends of one of these wires were connected with a universal discharger, whose knobs were placed within a quarter of an inch of each other; when the current of a Leyden battery was sent through the other wire, a secondary current was aroused in that connected with the discharger, which announced itself by a brilliant spark across the space separating the two knobs.

The wires here used were covered externally with a sheet of gutta-percha; and lest it should be supposed that a portion of the electricity of the battery had sprung from one wire to the other, two flat disks were taken. Each disk contained 75 feet of copper wire, wound in the form of a flat spiral, the successive convolutions of which were about two lines apart. One disk was placed upon the other one, the wire being so coiled that the convolutions of each disk constituted, so to say, the impress of those of the other, and the coils were separated from each other by a plate of varnished glass. The ends of one spiral were connected with the universal discharger, between whose knobs a thin platinum wire, ten inches long, was stretched. When the current of the Leyden battery was sent through the other spiral, the secondary current, evoked in the former, passed through the thin wire, and burnt it up with brilliant deflagration. A pair of spirals were next placed six inches apart, and a battery was discharged through one of them; the current aroused in the other was sufficient to deflagrate a thin platinum wire four inches in length.

We have every reason to suppose that the secondary current thus developed is of the same nature as the primary which produced it ; and hence we may infer, that if we conduct the secondary away and carry it through a second spiral, it, in its turn, will act the part of a primary, and evoke a *tertiary* current in a spiral brought near it. This was illustrated by experiment. First, two spirals were placed opposite to each other, through one of which the current of the battery was to be sent ; the other was that in which the secondary current was to be aroused. The ends of the latter were connected by wires with a third spiral placed at a distance, so that when the secondary current was excited it passes through the third spiral. Underneath the latter, and separated from it by a sheet of varnished glass, was a fourth spiral, whose two ends were connected with the universal discharger, between the knobs of which a quantity of gun-cotton was placed. When the battery was discharged through the first spiral, a secondary current was aroused in the second spiral, which completed its circuit by passing through the third spiral : here the secondary acted upon the spiral underneath, developed a tertiary current which was sufficiently strong to pass between the knobs, and to ignite the gun-cotton in its passage. It was shown that we might proceed in this way and cause the tertiary to excite a current of the fourth order, the latter a current of the fifth order, and so on ; these children, grandchildren, and great grandchildren of the primary being capable of producing all the effects of their wonderful progenitor.

The phenomena of the *extra current*, which exists for an instant contemporaneously with the ordinary current in a common voltaic spiral, were next exhibited ; and the question whether a spiral through which a Leyden battery was discharged exhibited any similar phenomena was submitted to examination. It was proved, that the electric discharge depended upon the *shape* of the circuit through which it passed : when two portions of such a circuit are brought near each other, so that the positive electricity passes in the same direction through both of them, the effect is that the discharge is *weaker* than if sent through a straight wire : if, on the contrary, the current flow through both portions in opposite directions the discharge is *stronger* than if it had passed through a straight wire. A flat spiral was taken, containing 75 feet of copper wire ; one end of the spiral was connected with a knob of the universal discharger, and the other knob was connected with the earth : between the knobs of the discharger about four inches of platinum wire were stretched ; on connecting the other end of the spiral with the battery a discharge passed through it of such a strength that it was quite unable to raise the platinum wire to the faintest glow. The same length of copper wire was then bent to and fro in a zigzag manner, so that on every two adjacent legs of the zigzag the current from the battery flowed in opposite directions. When these 75 feet of wire were interposed between the battery and the platinum wire, a discharge

precisely equal to that used in the former instance, raised the platinum wire to a high state of incandescence, and indeed could be made to destroy it altogether.

When a primary and a secondary spiral are placed opposite to each other, a peculiar reaction of the secondary upon the primary is observed. If the ends of a secondary (50 feet long) be connected by a thick wire, the effect upon the primary current is the same as when the ends of the secondary remain wholly unconnected. If the ends of the secondary be joined by a long thin platinum wire, the reaction of the secondary is such as to enfeeble the primary. This enfeeblement increases up to a certain limit as the resistance is increased, from which forwards it diminishes until it becomes insensible. This would appear to prove that to react upon the primary the secondary requires to be retarded; and that the greater the amount of the retardation, up to a certain limit, the greater is the enfeeblement. But by increasing the resistance we diminish the strength of the secondary, and when a certain limit is attained, this diminution is first compensated for by the influence of retardation, from which point forwards with every increase of the resistance, the enfeeblement of the primary is diminished. A primary current which fuses a certain length of platinum wire where the ends of the secondary are disunited, or where they are united by a thick wire, fails to do so when they are united with a thin wire. But if, instead of a thin wire, a body of much greater resistance, a column of water for example, be introduced, the platinum wire is fused as before.

[J. T.]

Friday, June 8.

THE DUKE OF NORTHUMBERLAND, K.G. F.R.S. President,
in the Chair.

PROFESSOR FARADAY, D.C.L. F.R.S.

On Ruhmkorff's Induction Apparatus.

THIS apparatus is known to consist of a soft iron core, intended to act magnetically, around which there is a coil of coarse copper wire, to be connected at pleasure with few or many cells of a voltaic battery; and external to this is a second coil of much thinner copper wire, having great length, in which the peculiar currents of the apparatus are to be produced. The coils of the wires are insulated from one another by a very careful mode of preparation. The inner, called the primary coil, is supplied with an automatic contrivance, so that when the battery is connected with it, the continuity is broken, to be renewed again an instant after; and thus a series of short intermitting currents, rapidly recurring, pass through it instead of one constant current. The outer coil, usually called the secondary, has its terminations apart; and these can be connected either metallically by a wire, or arranged with any interval or apparatus placed between them, in which effects of the induced current are to be shown, and by which its characters are to be examined.

When the secondary circuit is metallically complete, each brief current in the primary wire causes, according to well-known principles (*Exp. Res.* 10, &c.), two successive currents in opposite directions in the secondary wire; and if a galvanometer be included in the secondary circuit, it is seen that the communication of the primary wire with the battery is followed by a deflection of the needle in one direction, which then gradually swings to and fro, accompanied by curious spasmodic motions (which are understood upon a moment's inspection), until it comes to zero: if the primary current be then stopped, the galvanometer needle is deflected in the other direction, and after a few oscillations subsides quietly to zero again. The sum of the alternate induction currents having been thus shown to be equal in effect to zero, it was then explained how, if the secondary currents be interrupted in the smallest degree, even by the intervention of a hair or a piece of paper, all the currents of one kind, due to the beginnings of the short inducing currents in the primary wire, were stopped off from

the secondary wire (being expended in the primary wire itself), and only those due to the cessations of the primary currents left to show their power there; so that the secondary wire could then give a continuous series of intermitting (but not alternating) currents, all of which, therefore, had a common direction.

The remarkable character of the electricity of these currents was then shown and explained. Its intensity is such that it can strike across $\frac{1}{2}$ or $\frac{1}{4}$ an inch of air, whilst the intensity of the inducing current is so feeble that it cannot traverse any sensible striking distance: but it was also shown, that the more intense the electricity the less the sum of force transmitted in a given time by the action of the same battery and apparatus; and that when the interruption of the secondary circuit was the smallest possible, as by a hair's-breadth, the largest amount of electricity passed through the galvanometer connected with it. The power of the induced current to pass through six inches or more of rarified air, was shown in the form of Gassiot's cascade:* and the conversion of the dynamic force of the primary and inducing current into the static force of the induced electricity, was illustrated by the charging of electrometers and Leyden jars.

When the secondary current is interrupted, as just described, the inducing power of the primary current acts in its own wire to produce certain hurtful or wasteful results. Fizeau, by applying a Leyden jar (or its equivalent,) to the parts of the primary circuit near the contact breaker, took up this extra power at the moment of time, and converted it to a useful final purpose, upon principles belonging to static induction, the effects of which were briefly explained. Masson,† Grove,‡ and Sinstedden have made a like application to the terminals of the secondary wire; and Grove has pointed out striking changes in the character of the currents in it thus produced, and useful applications of the results. For instance, the spark in air between the ends of two platinum wires connected with the secondary terminals is flame-like, soft, and comparatively quiet compared with that which is produced when the terminals are respectively connected with the inner and outer coatings of a Leyden jar; for then it becomes very bright, sonorous, and apparently large, so that two sparks can hardly differ more than the same spark under these circumstances. The differences are even greater than the appearances show; for whilst the powerful rattling spark cannot fire wood, or paper, or even gunpowder, except by the use of expedients, the soft quiet spark at once inflames any of them. The effect of the static induction thus introduced is not so much to vary the *quantity* of electricity which

* Phil. Mag. 1854, vii. p. 99.

† Prize Essay, Haarlem Trans. 1854, pp. 46, 47.

‡ Phil. Mag. Jan. 1855, ix. p. 1.

passes, as the *time* of the passage. That electricity which, moving with comparative slowness through the great length of the secondary coil, produces a spark having sensible duration (and therefore in character like that of a Leyden jar passed through a wet thread,) is, when the jar is used, first employed in raising up a static induction charge, which when discharged produces a concentrated spark of no sensible duration, and therefore much more luminous and audible than the former. Fixing a piece of platinum wire horizontally across the ball of a Leyden jar, and then bringing the platinum wire secondary terminals respectively near its ends, two interruptions are produced in the secondary circuit, the sparks at which are like each other and equal in quantity of electricity, for the jar as yet forms only an insulating support. But if, in addition, either secondary terminal be connected by a wire with the outside of the jar, the spark on that side assumes the bright loud character before described, but ceases to fire gunpowder or wood; and no one would at first suppose, what is the truth, that there is the same electricity passing in one as in the other.

Another interesting effect of the static induction is the double spark. If one of the secondary terminals be connected with the outside of a Leyden jar, and the other be continued until near the knob or a wire connected with it, a soft spark appears at that interval for every successive current in the primary circuit. This spark, however, is double; for the electricity thrown into the jar at the moment of induction, is discharged back again at the same place the instant the induction is over; the first discharge heats and prepares the air there for the second discharge, and the two are so nearly simultaneous as to produce the appearance of a single spark to the unaided eye.

Reference was then made to the hopes raised by this instrument, of advance in the investigation of the magneto-electric power, by means of the great aid which it seems competent to supply. The results obtained by Grove* apparently referable to polarization were adverted to; as also the remarkable transverse bands presented in the recurring discharge across very rarified air†; and, founded as the instrument is by its core and its wires upon the joint effects of electro-dynamic and magneto-electric induction, it was observed that it gave great promise of aid in the investigation of that condition of either the space or the ether which is about magnets, and around every discharge of electricity, whether in good or bad conductors, and which is expressed by the terms (themselves synonymous) of the magnetic or the electrotonic state.

[M. F.]

* Phil. Trans. 1852, p. 93, &c.

† Phil. Mag. 1852, iv, p. 514.

Friday, January 25, 1856.

SIR BENJAMIN COLLINS BRODIE, D.C.L. F.R.S. Vice-President,
in the Chair.

W. R. GROVE, Esq. Q.C. F.R.S. M.R.I.

Inferences from the Negation of Perpetual Motion.

SCATTERED among the writings of philosophers will be found allusions to the subject of perpetual motion, and here and there are arguments like the following ; such a phenomenon cannot take place, or such a theory must be fallacious, because it involves the idea of perpetual motion : thus Dr. Roget advanced as an argument against the contact theory of electricity, as originally propounded, that if mere contact of dissimilar metals, without any chemical or molecular change, could produce electricity, then as electricity could, in its turn, be made to produce motion, we should thus get perpetual motion.

It may be well to define, as far as such a definition is possible, what is commonly meant by the term perpetual motion. In one sense, all motion, or rather all force, is perpetual ; for example, if a clock weight be wound up, it represents the force derived from the muscles of the arm which turns the key, the muscles again derive force indirectly from the chemical action of the food, and so on. As the weight descends it conveys motion to the wheels and pendulum ; the former giving force off in the form of heat from friction, the latter communicating motion to the air in contact with it, thence to the case of the clock, thence to the air of the room,—proved in a very simple manner by the ticking heard, which is in fact, a blow to the organ of hearing. Although ultimately lost to our senses, there is no reason to suppose that the force is ever in fact lost. The weight thus acting, reaches the ground quietly, and produces no effect at the termination of its course.

If, instead of being allowed to communicate its force to the works of the clock, the weight be allowed to descend suddenly, as by cutting the string by which it is suspended, it strikes the floor with a force which shakes the house ; and thus conveys, almost instantaneously, the amount of force which would be gradually dissipated, though not ultimately consumed, by the clock in a week or nine days.

This idea, however, of the perpetuity of force, is not what is

commonly understood by the term perpetual motion : that expression is used to convey the notion of a motive machine, the initial force of which is restored by the motion produced by itself,—a clock, so to speak, which winds itself up by its own wheels and pendulum, a pump which keeps itself going by the weight of the water which it has raised. Another notion, arising from a confusion between static and dynamic forces, was, that motion might be obtained without transferring force, as by a permanent magnet. All sound philosophers are of opinion that such effects are impossible; the work done by a given force, even assuming there were no such thing as friction, aerial resistance, &c., could never be more than equal to the initial force; the theoretical limit is equilibrium. The weight raised at one end of a lever can never, without the fresh application of extraneous force, raise the opposite weight which has produced its own elevation. A force can only produce motion when the resistance to it is less powerful than itself; if equal, it is equilibrium: thus if motion be produced, the resistance, being less than the initial or producing force, cannot reproduce this; for then the weaker would conquer the stronger force.

The object of this evening's communication was not, however, to adduce proofs that perpetual motion, in the sense above defined, is impossible; but assuming that as a recognised truth, to show certain consequences which had resulted, and others which were likely to result, from the negation of perpetual motion; and how this negation may be made a substantive and valuable aid to scientific investigation.

After Oersted made his discovery of electro-magnetism, philosophers of the highest attainments argued, that as a current of electricity, circulating in a wire round a bar of iron, produced magnetism, and as action and reaction are equal, and in contrary directions, a magnet placed within a spiral of wire should produce in the wire an electrical current: had it occurred to their minds that, if a permanent magnet could so produce electricity, and thence necessarily motion, they would thus get, in effect, perpetual motion, they would probably have anticipated the discovery of Faraday, and found that all that was required was to move the magnet with reference to the wire, and thus electricity might have been expected to be produced by a magnet without involving the supposed absurdity.

In a very different instance, viz. the expansion of water when freezing, not only heat, or the expansive force given to other bodies by a body cooling, would be given out by water freezing, but also the force due to the converse expansion in the body itself; and upon the argument that force would, in this case, be got out of nothing, Mr. J. Thomson saw that this supposed impossibility would not result if the freezing point of water were lowered by pressure, which was experimentally proved to be the case, by his brother.

In the effects of dilatation and contraction by heat and cold, when applied to produce mechanical effects, and consequently in the theory of the steam-engine, this subject possesses a greater practical interest. Watt supposed, that a given weight of water required the same quantity of what is termed total heat (that is, the sensible added to the latent heat) to keep it in the state of vapour, whatever was the pressure to which it was subjected, and consequently, however its expansive force varied. Clement Desormes was also supposed to have experimentally verified this law. If this were so, vapour raising a piston with a weight attached would produce mechanical power; and yet the same heat existing as at first, there would be no expenditure of the initial force; and if we suppose that the heat in the condenser was the real representative of the original heat, we should get perpetual motion. Southern supposed that the latent heat was constant, and that the heat of vapour under pressure increased as the sensible heat. M. Despretz, in 1832, made some experiments which led him to the conclusion that the increase was not in the same ratio as the sensible heat, but that yet there was an increase; a result confirmed and verified with great accuracy by M. Regnault, in some recent and elaborate researches. What seems to have occasioned the error in Watt and Clement Desormes' experiments was, the idea involved in the term latent heat; by which supposing the phenomenon of the disappearance of sensible heat to be due to the absorption of a material substance, that substance, 'caloric,' was thought to be restored when the vapour was condensed by water, even though the water was not subjected to pressure; but to estimate the total heat of vapour under pressure the vapour should be condensed while subjected to the same pressure as that under which it is generated, as was done in M. Despretz and M. Regnault's experiments.

Carnot's theory, that the mechanical force is produced by the transfer of heat, and that there is no ultimate cost or expenditure of heat in producing it, was founded in part on similar considerations; it is true that mechanical *motion* may be produced by the transfer of heat from a higher to a lower temperature, without ultimate loss, or, strictly speaking, with an infinitely small loss, but not, as he seemed to think, an available mechanical force, except upon an assumption which he did not make, and to which allusion will presently be made. Thus, let a weight be supposed to rest on a piston confining air of a certain temperature, say 50° , in a vessel non-conducting for heat; part of this temperature will be due to the pressure exerted, since compression produces heat in air, while dilatation produces cold. If the air be now heated, say to 70° , the piston, with the weight attached, will rise, and the temperature in consequence of the expansion of the air will cool somewhat, say to 69° , (the heat of friction of the piston may be taken to compensate the power lost by friction): if now a cold body be made to abstract 20° , the piston descending will, by its pressure, restore the 1° lost

by expansion; and when the piston has returned to its first position, the original 50° will remain as at first. Suppose this experiment repeated up to the rise of the piston; but when the piston is at its full elevation, and the cold body is applied, let the weight be removed, so as to drop upon a wheel, or to be used for other mechanical purposes, the descending piston will not now reach its original point without more heat being abstracted; from the removal of the weight there will not be the same force to restore the 1° , and the temperature will be 49° , or some fraction short of the original 50° ; if this were otherwise, then as the ball in falling may be made to produce heat by friction, we should have more heat than at first, or a creation of heat out of nothing, in other words, perpetual motion.

When force is abstracted from a thermal machine we ought to lose heat, if we suppose degrees of heat at a lower temperature to represent the same amount of force as the same number of degrees at a higher temperature; if, for instance, we suppose that a body cooling from 120° to 100° , gives off the same force as a body cooling from 20° to zero; this seems to be tacitly assumed by Carnot, but is probably not correct, the results of high-pressure steam, and other facts indicating a contrary conclusion. If then the 20° on the lower scale do not represent an equivalent force to the 20° on the higher, we *may* gain the same heat in degrees in the condenser as was lost from the furnace, and yet get derived power. There is frequently a confusion between the work performed which returns to the machine, and the derived work, or that which does not return, and is used for other purposes. This is puzzling to the reader of treatises on the steam-engine, and kindred subjects, and has led to much obscurity of thought and expression.

M. Seguin, in 1839, controverted the position that derived power could be got by the mere transfer of heat, and by calculation from certain known data, such as the law of Mariotte, viz. that the elastic force of gases and vapours increased directly with the pressure, and assuming that for vapour between 100° and 150° centigrade each degree of elevation of temperature was produced by a thermal unit, he deduced the equivalent of mechanical work capable of being performed by a given decrement of heat; and thus concluded that for ordinary pressures about one gramme of water losing one degree centigrade would produce a force capable of raising a weight of 500 grammes through a space of one metre; this estimate is a little beyond that given by the more recent experiments of Mr. Joule. M. Seguin has, however, since the accurate and elaborate experiments of M. Regnault, necessarily varied his estimate, as by these experiments it appears that, within certain limits, for elevating the temperature of compressed vapour by one degree, no more than about $\frac{1}{10}$ ths of a degree of total heat is required; consequently, the equivalent multiplied in this ratio would be 1666 grammes, instead of 500. Other investigators have given numbers more or less dis-

cordant; so that without giving any opinion on their different results, this question may be considered at present far from settled. M. Regnault himself does not give the law by which the ratio of heat varies with reference to the pressure, and is still believed to be engaged in researches on the subject, one involving questions of which experiments on the mechanical effects of elastic fluids seem to offer the most promising means of solution.

One of the greatest difficulties which had presented itself to Mr. Grove's mind, with reference to the theory of Carnot, had been one of analogy, derived from the received theories of electricity. Many electrical cases might be cited in which no electricity is supposed to be lost, though a certain mechanical effort is produced by the electricity; if, for instance, a ball vibrates between a positively and negatively electrified substance, none of our electrical theories lead us to believe that any difference in the actual amount of electricity transferred would be occasioned by the ball being attached to a lever which would strike a wheel or produce any other mechanical effect.

In preparing this evening's communication an experiment had occurred to him, which, though performed with imperfect apparatus and therefore requiring verification, does, as far as it goes, support the view derived from the negation of perpetual motion, viz. that when electricity performs any mechanical work which does not return to the machine, electrical power is lost. The experiment is made in the following manner. A Leyden jar of one square foot coated surface has its interior connected with a Cuthbertson's electrometer, between which and the outer coating of the jar are a pair of discharging balls fixed at a certain distance (about $\frac{1}{4}$ an inch apart). Between the Leyden jar and the prime conductor is inserted a small unit jar of nine square inches surface, the knobs of which are 0.2 inch apart.

The balance of the electrometer is now fixed by a stiff wire inserted between the attracting knobs, and the Leyden jar charged by discharges from the unit jar. After a certain number of these, (22 in the experiment performed in the theatre on this occasion,) the discharge of the large jar takes place across the $\frac{1}{4}$ -inch interval; this may be viewed as the expression of electrical power received from the unit jar. The experiment is now repeated, the wire between the balls having been removed, and therefore the 'tip' or the raising of the weight, is performed by the electrical repulsion and attraction of the two pairs of balls; at 22 discharges of the unit jar the balance is subverted, and one knob drops upon the other, but *no discharge takes place*, showing that some electricity has been lost, or converted into the mechanical power which raises the balance. By another mode of expression the electricity may be supposed to be masked or analogous to latent heat, and would be restored if the ball were brought back, without discharge, by extraneous force.

This experiment has succeeded in so large an average of cases, and so responds to theory, that, notwithstanding the imperfection of the apparatus, Mr. Grove places much reliance on it; indeed, it is difficult to see, if the discharges or other electrical effects were the same in both cases, why the raising the ball, being extra, and the ball being capable by its fall of producing electricity or other force, force would not thus be got out of nothing, or perpetual motion attained.

The experiment is believed to be new, and to be suggestive of others of a similar character, which may be indefinitely varied. Thus, two balls made to diverge by electricity should not give to an electrometer the same amount of electricity as if they were, whilst electrified, kept forcibly together, an experiment which may be tried by Coulomb's torsion balance.

There is an advantage in electrical experiments of this class, as compared with those on heat, viz. that though there is no perfect insulation for electricity, yet our means of insulation are immeasurably superior to any attainable for heat.

Similar reasoning might be applied to other forces; and many cases, bearing on this subject, have been considered by Mr. Grove in his essay on the "Correlation of Physical Forces."

Certain objections to these views were then discussed, and especially some apparently formidable ones presented by M. Matteucci in a paper published by him some time ago.*

This distinguished philosopher cites the fact, that a voltaic battery decomposing water in a voltameter, while the same current is employed at the same time to make an electro-magnet, nevertheless gives in the voltameter an equivalent of gas, or decomposed substance, for each equivalent of chemical decomposition in the cells, and will give the same ratios if the electro-magnet be removed. In answer to this objection it may be said, that in the circumstances under which this experiment is ordinarily performed, several cells of the battery are used, and so there is a far greater amount of force generated in the cells than is indicated by the effect in the voltameter. If, moreover, the magnet is not interposed, still the magnetic force is equally existent through the whole circuit; for instance, the wires joining the plates will attract iron filings, deflect magnetic needles, &c. By the iron core a small portion of the force is absorbed while it is being made a magnet, but this ceases to be absorbed when the magnet is made; this is proved by the recent observations of Mr. Latimer Clarke, which were fully entered into and extended by Mr. Faraday, in a lecture at the Institution (Jan. 20, 1854†). It is like the case of a pulley and weight, which latter exhausts force while it is being raised, but when raised the force is free, and may be used for other purposes.

* Archives des Sciences Physiques, Vol. IV., p. 380.

† Proceedings of the Royal Institution, Vol. I., p. 345.

If a battery of one cell, just capable of decomposing water and no more, be employed, this will cease to decompose while making a magnet. There must, in every case, be preponderating chemical affinity in the battery cells, either by the nature of its elements or by the reduplication of series, to effect decomposition in the voltameter, and if the point is just reached at which this is effected, and the power is then reduced by any resistance, decomposition ceases : were it otherwise, were the decomposition in the voltameter the exponent of the entire force of the generating cells, and these could independently produce magnetic force, this latter force would be got from nothing, and perpetual motion be obtained.

In another case, cited by M. Matteucci, viz. that a piece of zinc dissolved in dilute sulphuric acid gives somewhat less heat than when the zinc has a wire of platinum attached to it, and is dissolved by the same quantity of acid, the argument is deduced that as there is more electricity in the second than in the first case, there should be less heat ; but, as according to our received theories the heat is a product of the electric current, and in consequence of the impurity of zinc, electricity is generated in the first case molecularly in what is called local action, though not thrown into a general direction, there should be more of both heat and electricity in the second than in the first case ; as the heat and electricity due to the voltaic combination of zinc and platinum are added to that excited on the surface of the zinc, and the zinc should be, as in fact it is, more rapidly dissolved. Other instances are given by M. Matteucci, and many additional cases of a similar description might be suggested. But although it is difficult, perhaps impossible, to restrict the action of any one force to the production of one other force, and one only ; yet if the whole of one force, say chemical action, be supposed to be employed in producing its full equivalent of another force, say heat ; then as this heat is capable in its turn of reproducing chemical action, and, in the limit, a quantity equal to or at least only infinitesimally less than the initial force ; if this could at the same time produce independently another force, say magnetism, we could, by adding this to the total heat, get more than the original chemical action, and thus create force or obtain perpetual motion.

The impossibility of perpetual motion thus becomes a valuable test of the approach that in any experiment we may have made to eliminating the whole power which a given natural force is capable of producing ; it also serves, when any new natural phenomenon is discovered, to enable us to ascertain how far this can be brought into relation with those previously known. Thus when Moser discovered that dissimilar metals would impress each other respectively with a faint image of their superficial inequalities ; that, for instance, a copper coin placed on a polished silver plate, even in the dark, would, after a short time, leave on the silver plate an impression of its own device, it occurred to Mr. Grove that as this experiment showed a

physical radiation taking place between the metals, it would afford a reason for the effects produced in Volta's contact experiment, without supposing a force without consumption or change in the matter evolving it. This led him to try the effect of closely approximating discs of zinc and copper without bringing them into metallic contact; and it was found that discs thus approximated, and then quickly separated, affected the electroscope just as though they had been brought into contact. Without giving any opinion as to what may be the nature of the radiation in Moser's phenomena, this experiment removes the difficulty presented by that of Volta to the chemical theory of electricity.

The general scope of the argument from the negation of perpetual motion leads the mind to regard the so-called imponderables as modes of motion, and not as different kinds or species of matter; the recent progress of science is continually tending to get rid of the hypotheses of fluids, of occult qualities, or latent entities, which might have been necessary in an earlier stage of scientific enquiry, and from which it is now extremely difficult to emancipate the mind: but if we can, as it is to be hoped we shall, ultimately arrive at a general dynamic theory, by which the known laws of motion of masses can be applied to molecules, or the minute structural parts of matter, it seems scarcely conceivable that the mind of man can further simplify the means of comprehending natural phenomena.

[W. R. G.]

Friday, February 1.

THE DUKE OF NORTHUMBERLAND, K.G. F.R.S. President,
in the Chair.

PROFESSOR TYNDALL, F.R.S.

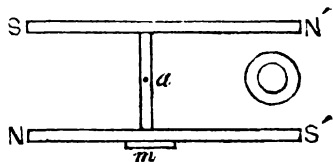
*On the Disposition of Force in Paramagnetic and
Diamagnetic Bodies.*

THE notion of an attractive force, which draws bodies towards the centre of the earth, was entertained by Anaxagoras and his pupils, by Democritus, Pythagoras, and Epicurus; and the conjectures of these ancients were renewed by Galileo, Huyghens, and others, who stated that bodies attract each other as a magnet attracts iron. Kepler applied the notion to bodies beyond the surface of the earth, and affirmed the extension of this force to the most distant stars. Thus it would appear, that in the attraction of iron by a magnet originated the conception of the force of gravitation. Nevertheless, if we look closely at the matter, it will be seen that the magnetic force possesses characters strikingly distinct from those of the force which holds the universe together. The theory of gravitation is, that every particle of matter attracts every other particle; in magnetism also we have the phenomenon of attraction, but we have also, at the same time, the fact of repulsion, and the final effect is always due to the difference of these two forces. A body may be intensely acted on by a magnet, and still no motion of translation will follow, if the repulsion be equal to the attraction. A dipping needle was exhibited: previous to magnetization, the needle, when its centre of gravity was supported, stood accurately level; but, after magnetization, one end of it was pulled towards the north pole of the earth. The needle, however, being suspended from the arm of a fine balance, it was shown that its *weight* was unaltered by its magnetization. In like manner, when the needle was permitted to float upon a liquid, and thus to follow the attraction of the north magnetic pole of the earth, there was no motion of the mass towards the pole referred to; and the reason was known to be, that although the marked end of the needle was *attracted* by the north pole, the unmarked end was *repelled* by an equal quantity, and these two equal and opposite forces neutralized each other as regards the production of a motion of translation. When the pole of an ordinary magnet was brought to act upon the swimming needle, the latter was attracted,—the reason being that the attracted end of the needle being much nearer to the pole of the magnet than the repelled end, the

force of attraction was the more powerful of the two ; but in the case of the earth, the pole being so distant, the length of the needle was practically zero. In like manner, when a piece of iron is presented to a magnet, the nearer parts are attracted, while the more distant parts are repelled ; and because the attracted portions are nearer to the magnet than the repelled ones, we have a balance in favour of attraction. Here then is the most wonderful characteristic of the magnetic force, which distinguishes it from that of gravitation. The latter is a simple unipolar force, while the former is duplex or polar. Were gravitation like magnetism, a stone would no more fall to the ground than a piece of iron towards the north magnetic pole : and thus, however rich in consequences the supposition of Kepler and others may have been, it was clear that a force like that of magnetism would not be able to transact the business of the universe.

The object of the evening's discourse was to inquire whether the force of diamagnetism, which manifested itself as a repulsion of certain bodies by the poles of a magnet, was to be ranged as a polar force, beside that of magnetism ; or as an unipolar force, beside that of gravitation. When a cylinder of soft iron is placed within a helix, and surrounded by an electric current, the antithesis of its two ends, or in other words, its polar excitation, is at once manifested by its action upon a magnetic needle ; and it may be asked why a cylinder of bismuth may not be substituted for the cylinder of iron, and its state similarly examined. The reason is, that the excitement of the bismuth is so feeble, that it would be quite masked by that of the helix in which it is enclosed ; and the problem that now meets us is, so to excite a diamagnetic body that the pure action of the body upon a magnetic needle may be observed, unmixed with the action of the body used to excite the diamagnetic.

How this may be effected, was illustrated in the following manner :—an upright helix of covered copper wire was placed upon the table, and it was shown that the top of the helix attracted, while its bottom repelled the same pole of a magnetic needle ; its central point, on the contrary, was neutral, and exhibited neither attraction nor repulsion. This helix was caused to stand between the two poles $N'S'$ of an astatic magnet ; the two magnets $S N'$ and $S'N$ were united by a rigid cross piece at



their centres, and suspended from the point a , so that both magnets swung in the same horizontal plane. It was so arranged that the poles $N'S'$ were opposite to the central or neutral point of the helix, so that when a current was sent through the latter, the magnets were unaffected by the current. Here then we had an excited helix which itself had no action upon the magnets, and we were thus at liberty to examine the action of a body placed within the helix and excited by it, undisturbed by the influence of the latter. The helix was 12 inches

high, and a cylinder of soft iron 6 inches long suspended from a string and passing over a pulley could be raised or lowered within the helix. When it was so far sunk that its lower end rested upon the table, the upper end found itself between the poles $N S$ attracting one of them, and repelling the other, and consequently deflecting the astatic system in a certain direction. When the cylinder was raised so that the upper end was at the level of the top of the helix, its lower end was between the poles $N' S'$; and a deflection opposed in direction to the former one was the immediate consequence. To render these deflections more visible to the audience, a mirror m , was attached to the system of magnets; a beam of light thrown upon the mirror was reflected and projected as a bright disk against the wall of the theatre; the distance of this image from the mirror being considerable, and its angular motion double that of the latter, a very slight motion of the magnet was sufficient to produce a displacement of the image through several yards. This then is the principle of the beautiful apparatus* by which the investigation now brought forward was conducted. It is manifest that if a second helix be placed between the poles $S N$ with a cylinder within it, the action upon the astatic magnet may be exalted. This was the arrangement made use of in the actual inquiry. Thus to intensify the feeble action, which it is here our object to seek, we have in the first place neutralized the action of the earth upon the magnets, by placing them astatically. Secondly, by making use of two cylinders, and permitting them to act simultaneously on the four poles of the magnets, we have rendered the deflecting force four times what it would be, supposing only a single pole to be used. Finally, the whole apparatus was enclosed in a suitable case, which protected the magnets from atmospheric currents, and the deflections were read off through a glass plate in the case, by means of a telescope and scale placed at a considerable distance from the instrument.

A pair of bismuth cylinders was first examined. Sending a current through the helices, and observing that the magnets swung perfectly free, it was first arranged that the cylinders within the helices had their central points opposite to the poles of the magnets. All being at rest the number on the scale marked by the cross wire of the telescope was 572. The cylinders were then moved so that two ends were brought to bear simultaneously upon the magnetic poles: the magnet moved promptly, and after some oscillations† came to rest at the number 612; thus moving from a smaller to a larger number. The other two ends of the bars were next brought to bear upon the magnet: a prompt deflection was the consequence, and the final position of equilibrium was 526; the movement being from a larger to a smaller number. We thus observe a manifest

* Devised by Prof. W. Weber, and constructed by M. Leyser, of Leipzig.

† To lessen these a copper damper was made use of.

polar action of the bismuth cylinders upon the magnet; one pair of ends deflecting it in one direction, and the other pair deflecting it in the opposite direction.

Substituting for the cylinders of bismuth thin cylinders of iron, of magnetic slate, of sulphate of iron, carbonate of iron, protochloride of iron, red ferrocyanide of potassium, and other magnetic bodies, it was found that when the position of the magnetic cylinders was the same as that of the cylinders of bismuth, the deflection produced by the former was always opposed in direction to that produced by the latter; and hence the disposition of the force in the diamagnetic body must have been precisely antithetical to its disposition in the magnetic ones.

But it will be urged, and indeed has been urged against this inference, that the deflection produced by the bismuth cylinders is purely due to the currents of induction excited in the mass by its motion within the helices. In reply to this objection, it may be stated, in the first place, that the deflection is permanent, and cannot therefore be due to induced currents, which are only of momentary duration. It has also been urged that such experiments ought to be made with other metals, and with better conductors than bismuth, for if due to currents of induction the better the conductor the more exalted will be the effect. This requirement was complied with.

Cylinders of antimony were substituted for those of bismuth. This metal is a better conductor of electricity, but less strongly diamagnetic than bismuth. If therefore the action referred to be due to induced currents we ought to have it greater in the case of antimony than with bismuth; but if it springs from a true diamagnetic polarity, the action of the bismuth ought to exceed that of the antimony. Experiment proves that the latter is the case, and that hence the deflection produced by these metals is due to their diamagnetic, and not to their conductive capacity. Copper cylinders were next examined: here we have a metal which conducts electricity fifty times better than bismuth, but its diamagnetic power is nearly null; if the effects be due to induction we ought to have them here in an enormously exaggerated degree, but no sensible deflection was produced by the two cylinders of copper.

It has also been proposed by the opponents of diamagnetic polarity to coat fragments of bismuth with some insulating substance, so as to render the formation of induced currents impossible, and to test the question with cylinders of these fragments. This requirement was also fulfilled. It is only necessary to reduce the bismuth to powder and expose it for a short time to the air to cause the particles to become so far oxidised as to render them perfectly insulating. The power of the powder in this respect was exhibited experimentally in the lecture; nevertheless this powder, enclosed in glass tubes, exhibited an action scarcely less powerful than that of the massive cylinders.

But the most rigid proof, a proof admitted to be conclusive by

those who have denied the antithesis of magnetism and diamagnetism, remains to be stated. Prisms of the same heavy glass as that with which the diamagnetic force was discovered, were substituted for the metallic cylinders, and their action upon the magnet was proved to be precisely the same in kind as that of the cylinders of bismuth. The inquiry was also extended to other insulators: to phosphorus, sulphur, nitre, calcareous spar, statuary marble, with the same invariable result: each of these substances was proved polar, the disposition of the force being the same as that of bismuth and the reverse of that of iron. When a bar of iron is set erect, its lower end is known to be a north pole, and its upper end a south pole, in virtue of the earth's induction. A marble statue, on the contrary, has its feet a south pole, and its head a north pole, and there is no doubt that the same remark applies to its living archetype; each man walking over the earth's surface is a true diamagnet, with its poles the reverse of those of a mass of magnetic matter of the same shape and in a similar position.

An experiment of practical value, as affording a ready estimate of the different conductive powers of two metals for electricity, was exhibited, for the purpose of proving experimentally some of the assertions made by the speaker in reference to this subject. A cube of bismuth was taken and suspended by a twisted string between the two poles of an electro-magnet. The cube was attached by a short copper wire to a little square pyramid, the base of which was horizontal, and its sides formed of four small triangular pieces of looking-glass. A beam of light was suffered to fall upon this reflector, and as the reflector followed the motion of the cube the images cast from its sides followed each other in succession, each describing a circle of about 30 feet in diameter. As the velocity of rotation augmented, these images blended into a continuous ring of light. At a particular instant the electro-magnet was excited, currents were evolved in the rotating cube, and the strength of these currents, which increases with the conductivity of the cube for electricity, was practically estimated by the time required to bring the cube and its associated mirrors to a state of rest. With bismuth this time amounted to a score of seconds or more: a cube of copper, on the contrary, was struck almost instantly motionless when the circuit was established.

[J. T.]

Friday, February 22.

SIR BENJAMIN COLLINS BRODIE, Bart. D.C.L. F.R.S.
Vice-President, in the Chair.

MR. FARADAY, D.C.L. F.R.S.

On certain Magnetic Actions and Affections.

ALL bodies subject to magnetic induction, when placed in the ordinary magnetic field between the poles of a magnet, are affected; paramagnetic bodies tend to pass bodily from weaker to stronger places of force, and diamagnetic bodies from stronger to weaker places of force. If the bodies are elongated, then those that are paramagnetic set along the lines of force, and those that are diamagnetic across them: but if these bodies have a spherical form, are amorphous, and are perfectly free from permanent magnetic charge, they have no tendency to set in a particular direction. Nevertheless, there are bodies of both classes, which being *crystalline*, have the power of setting when a single crystal is wrought into the form of a sphere, and these are called magne-crystals; their number is increasing continually; carbonate of lime, bismuth, tourmaline, &c., are of this nature. Bodies which being magnetic, set, because they are elongated, are greatly influenced in the force of the set by the nature of the medium surrounding them, and to such an extent that they not merely vary in their force from the maximum to nothing, but will often set axially in one medium, and equatorially in another. Yet the same bodies, if magne-crystalline and formed into spheres, though they set well in the magnetic field, will set with the *same* force whatever the change in the media about them, and are perfectly freed from the influence of the latter. Thus, if a crystal of bismuth formed into a sphere, or a vertical cylinder, has, when suspended, its magne-crystalline axis horizontal, and if the various media about it, from saturated solution of sulphate of iron, up to phosphorus, through air, water, alcohol, oil, be changed one for another, no alteration in the amount of torsion force required to displace the magne-crystal will occur, provided the force of the magnet be constant, notwithstanding that the list of media includes highly paramagnetic and diamagnetic bodies; and in such cases the measurement of the power of set is relieved from a multitude of interfering circumstances existing in other cases, and that power which is dependent upon the internal structure and con-

dition of the substance is proved to be, at the same temperature, always the same.

A consequence of magne-crystallic structure is that the same body is more paramagnetic, or more diamagnetic in one direction than in another; and therefore it follows, that though such a crystal may have no variation in set-force, produced by change of the surrounding medium, it may have a variation produced in the absolute force of attraction or repulsion; even up to the point of being attracted in one position and repelled in another, though no change in form, or in the surrounding medium, or in the force of the magnet, or in the nature of the body itself, be made, but simply a change in the *direction of the structure*. This was shown by a crystal of the red ferropotassiate of potassa, which, being coated carefully with wax, was suspended from the arm of a torsion balance so that it dipped into a solution of proto-sulphate of iron occupying the magnetic field.* When the magne-crystallic axis was parallel to the lines of force the crystal was attracted by the magnetic pole, when it was perpendicular to the lines of force the crystal was repelled; acting like a paramagnetic and a diamagnetic in turns. No magne-crystal has yet been found having such a relation to a vacuum, or to carbonic acid (its magnetic equivalent); calcareous spar is nearly coincident with such a medium, and shows different degrees of force in the two directions, but is always a little on the diamagnetic side. Calcareous spar having a trace of iron has been found very nearly up to the desired point, on the paramagnetic side; and as these preserve the full magne-crystallic relation of the two directions, there is no reason to suppose that a crystal may not be found which may not be paramagnetic in one direction, and diamagnetic in another, in respect of space as zero.

There is every reason to believe that the general magnetic relations of a magne-crystal are the same with those of the same substance in the amorphous state; and that the circumstances which influence one, influence the other to the same degree. In that case, the magnetic affections of a body might be ascertained by the examination of the magne-crystallic affections; thus the effect of heat upon bismuth, tourmaline, &c., might be examined by the set of the crystals; and with so much the greater advantage, that short globular forms could be used, perfectly free from the magnetic influence of the surrounding media required as temperature baths, and requiring no displacement of these media with the motion of the crystal. So crystals of bismuth, tourmaline, carbonate of iron, and other bodies, were suspended in baths of oil, water, &c., the temperature gradually raised and lowered, and the torsion force of the set for each temperature observed. With bismuth, a crystal having a force of 200 at 20° F. was reduced to a force of 70 at 300°, and the diminution of force appeared to be nearly equal in all parts of the scale for an

* 2½ volumes of saturated solution, at 65° F., and 1 volume of water.

equal number of degrees. A piece of amorphous bismuth, compressed in one direction, gave nearly the same amount and degree of change for the same alteration of temperature; leading us to the persuasion that the whole magnetic force of bismuth as a diamagnetic body would suffer like change. A crystal of tourmaline, which at 0° had a setting force of 540, when raised to 300° , had a setting force of only 270: the loss of force was progressive, being greater at lower than at high temperatures; for a change from 0° to 30° caused a loss of force equal 50, whilst a change from 270° to 300° , caused a loss of only 20. Carbonate of iron suffered a like change; at 0° the force was 1140, at 300° it was only 415; at the lower temperature the loss for 30° was 120 of force, at the upper it was only 34.

In all these and in many other cases, both with paramagnetic and diamagnetic bodies, the magne-crystallic differences diminished with the elevation of temperature; and therefore it may be considered probable, that the actual magnetic force changed in the same direction. But on extending the results to iron, nickel, and cobalt, employing these metals as very small prisms associated with copper cubes to give them weight, it was found that another result occurred. Iron, whether at the temperature of 30° or 300° , or any intermediate degree, underwent no change of force, it remained at 300, which was the expression for the piece employed under the circumstances. We know that at higher temperatures it loses power, and that at a bright red it is almost destitute of inductive magnetic force. A piece of nickel, which at 95° had a setting power of 300, when raised to 285° , had a power of only 290, so that it had lost a thirtieth part of its force; at the heat of boiling oil, it is known to lose nearly all its force, being unable then to affect a magnetic needle. Cobalt, on the other hand, requires a far higher temperature than iron to remove its magnetic character, a heat near that of melting copper being necessary. As to lower temperatures it was found, that an elevation from 70° to 300° caused an absolute *increase* of the magnetic force from 293 to 333. It is evident, therefore, that there is a certain temperature, or range of temperature above 300° , at which the magnetic force of cobalt is a maximum; and that elevation above, or depression below that temperature causes a diminution of the force. The case is probably the same for iron; its maximum magnetic force occurring at temperatures between 0° and 300° . If nickel is subject to the same conditions of a maximum, then that state must come on at temperatures below 0° : and it may be further remarked, that as the maximum conditions occur in the following order for ascending temperatures, nickel, iron, cobalt, such also is the same order for the temperatures at which they lose their high and distinctive magnetic place amongst metals.

[M. F.]

Friday, February 29.

SIR HENRY HOLLAND, M.D. F.R.S. Vice-President,
in the Chair.

PROFESSOR WM. THOMSON, F.R.S.

On the Origin and Transformations of Motive Power.

THE speaker commenced by referring to the term *work done*, as applied to the action of a force pressing against a body which yields, and, to the term *mechanical effect produced*, which may be either applied to a resisting force overcome, or to matter set in motion. Often the mechanical effect of work done consists in a combination of those two classes of effects. It was pointed out that a careful study of nature leads to no firmer conviction than that work cannot be done without producing an indestructible equivalent of mechanical effect. Various familiar instances of an apparent loss of mechanical effect, as in the friction, impact, cutting, or bending of solids, were alluded to, but especially that which is presented by a fluid in motion. Although in hammering solids, or in forcing solids to slide against one another, it may have been supposed that the alterations which the solids experience from such processes constitute the effects mechanically equivalent to the work spent, no such explanation can be contemplated for the case of work spent in agitating a fluid. If water in a basin be stirred round and left revolving, after a few minutes it may be observed to have lost all sensible or otherwise discernible signs of motion. Yet it has not communicated motion to other matter round it; and it appears as if it has retained no effect whatever from the state of motion in which it had been. It is not tolerable to suppose that its motion can have come to nothing; and until fourteen years ago confession of ignorance and expectation of light was all that philosophy taught regarding the vast class of natural phenomena, of which the case alluded to is an example. Mayer, in 1842, and Joule, in 1843, asserted that heat is the equivalent obtained for work spent in agitating a fluid, and both gave good reasons in support of their assertion. Many observations have been cited to prove that heat is not generated by the friction of fluids: but that heat is generated by the friction of fluids has been established beyond all doubt by the powerful and refined tests applied by Joule in his experimental investigation of the subject.

An instrument was exhibited, by means of which the temperature of a small quantity of water, contained in a shallow circular case provided with vanes in its top and bottom, and violently agitated by a circular disc provided with similar vanes, and made to turn rapidly round, could easily be raised in temperature several degrees in a few minutes by the power of a man, and by means of which steam power applied to turn the disc had raised the temperature of the water by 30° in half an hour. The bearings of the shaft, to the end of which the disc was attached, were entirely external; so that there was no friction of solids under the water, and no way of accounting for the heat developed except by the friction in the fluid itself.

It was pointed out that the heat thus obtained is not *produced from a source*, but is *generated*; and that what is called into existence by the work of a man's arm cannot be matter.

Davy's experiment, in which two pieces of ice were melted by rubbing them together in an atmosphere below the freezing point, was referred to as the first completed experimental demonstration of the immateriality of heat, although not so simple a demonstration as Joule's, and although Davy himself gives only defective reasoning to establish the true conclusion which he draws from it. Rumford's inquiry concerning the "Source of the Heat which is excited by Friction" was referred to as only wanting an easy additional experiment—a comparison of the thermal effects of dissolving (in an acid for instance), or of burning, the powder obtained by rubbing together solids, with the thermal effects obtained by dissolving or burning an equal weight of the same substance or substances in one mass or in large fragments—to prove that the heat developed by the friction is not *produced from the solids*, but is *called into existence between them*. An unfortunate use of the word "capacity for heat," which has been the occasion of much confusion ever since the discovery of latent heat, and has frequently obstructed the natural course of reasoning on thermal and thermo-dynamic phenomena, appears to have led both Rumford and Davy to give reasoning which no one could for a moment feel to be conclusive, and to have prevented each from giving a demonstration which would have established once and for ever the immateriality of heat.

Another case of apparent loss of work, well known to an audience in the Royal Institution—that in which a mass of copper is compelled to move in the neighbourhood of a magnet—was adduced; and an experiment was made to demonstrate that in it also heat appears as an effect of the work which has been spent. A copper ball, about an inch in diameter, was forced to rotate rapidly between the poles of a powerful electro-magnet. After about a minute it was found by a thermometer to have risen by 15° Fahr. After the rotation was continued for a few minutes more, and again stopped, the ball was found to be so hot that a piece of phosphorus applied to any point of its surface immediately took fire. It is

clear that in this experiment the electric currents, discovered by Faraday to be induced in the copper in virtue of its motion in the neighbourhood of the magnet, generated the heat which became sensible. Joule first raised the question, Is any heat generated by an induced electric current in the locality of the inductive action? He not only made experiments which established an affirmative answer to that question, but he used the mode of generating heat by mechanical work established by those experiments, as a way of finding the numerical relation between units of heat and units of work, and so first arrived at a determination of the mechanical value of heat. At the same time (1843) he gave another determination founded on the friction of fluids in motion; and six years later he gave the best determination yet obtained, according to which it appears that 772 foot pounds of work, (that is 772 times the amount of work required to overcome a force equal to the weight of 1 lb. through a space of 1 foot,) is required to generate as much heat as will raise the temperature of a pound of water by one degree.

The reverse transformation of heat into mechanical work was next considered, and the working of a steam-engine was referred to as an illustration. An original model of Stirling's air-engine was shown in operation, developing motive power from heat supplied to it by a spirit lamp, by means of the alternate contractions and expansions of one mass of air. Thermo-electric currents, and common mechanical action produced by them, were referred to as illustrating another very distinct class of means by which the same transformation may be effected. It was pointed out that in each case, while heat is taken in by the material arrangement or machine, from the source of heat, heat is always given out in another locality, which is at a lower temperature than the locality at which heat is taken in. But it was remarked that the quantity of heat given out is not; (as Carnot pointed out, it would be if heat were a substance,) the same as the quantity of heat taken in, but, as Joule insisted, less than the quantity taken in by an amount mechanically equivalent to the motive power developed. The modification of Carnot's theory to adapt it to this truth was alluded to; and the great distinction which it leads to between reversible and not reversible transformations of motive power was only mentioned.

To facilitate farther statements regarding transformations of motive power, certain terms, introduced to designate various forms under which it is manifested, were explained. Any piece of matter, or any group of bodies, however connected, which either is in motion, or can get into motion without external assistance, has what is called mechanical energy. The energy of motion may be called either "dynamical energy," or "actual energy." The energy of a material system at rest, in virtue of which it can get into motion, is called "potential energy," or, generally, motive power possessed among different pieces of matter, in virtue of their relative

positions, is called potential energy. To show the use of these terms, and explain the ideas of a *store of energy*, and of conversions and transformations of energy, various illustrations were adduced. A stone at a height, or an elevated reservoir of water, has potential energy. If the stone be let fall, its potential energy is converted into actual energy during its descent, exists entirely as the actual energy of its own motion at the instant before it strikes, and is transformed into heat at the moment of coming to rest on the ground. If the water flow down by a gradual channel, its potential energy is gradually converted into heat by fluid friction, and the fluid becomes warmer by a degree Fahr. for every 772 feet of the descent. There is potential energy, and there is dynamical energy, between the earth and the sun. There is most potential energy and least actual energy in July, when they are at their greatest distance asunder, and when their relative motion is slowest. There is least potential energy and most dynamical energy in January, when they are at their least distance, and when their relative motion is most rapid. The gain of dynamical energy from the one time to the other is equal to the loss of potential energy.

Potential energy of gravitation is possessed by every two pieces of matter at a distance from one another; but there is also potential energy in the mutual action of contiguous particles in a spring when bent, or in an elastic cord when stretched.

There is potential energy of electric force in any distribution of electricity, or among any group of electrified bodies. There is potential energy of magnetic force between the different parts of a steel magnet, or between different steel magnets, or between a magnet and a body of any substance of either paramagnetic or diamagnetic inductive capacity. There is potential energy of chemical force between any two substances which have what is called affinity for one another,—for instance, between fuel and oxygen, between food and oxygen, between zinc in a galvanic battery and oxygen. There is potential energy of chemical force among the different ingredients of gunpowder or gun cotton. There is potential energy of what may be called chemical force, among the particles of soft phosphorus, which is spent in the allotropic transformation into red phosphorus; and among the particles of prismatically crystallized sulphur, which is spent when the substance assumes the octahedral crystallization.

To make chemical combination take place without generating its equivalent of heat, all that is necessary is to resist the chemical force operating in the combination, and take up its effect in some other form of energy than heat. In a series of admirable researches on the agency of electricity in transformations of energy,* Joule

* On the Production of Heat by Voltaic Electricity," communicated to the Royal Society Dec. 17, 1840, (see Proceedings of that date,) and published *Phil. Mag.* Oct. 1841. [On

showed that the chemical combinations taking place in a galvanic battery may be directed to produce a large, probably in some forms of battery an unlimited, proportion of their heat, not in the locality of combination, but in a metallic wire at any distance from that locality; or that they may be directed not to generate that part of their heat at all, but instead to raise weights, by means of a rotating engine driven by the current. Thus if we allow zinc to combine with oxygen by the beautiful process which Grove has given in his battery, we find developed in a wire connecting the two poles the heat which would have appeared directly if the zinc had been burned in oxygen gas; or if we make the current drive a galvanic engine, we have, in weights raised, an equivalent of potential energy for the potential energy between zinc and oxygen spent in the combination.

The economic relations between the electric and the thermodynamic method of transformation from chemical affinity to available motive power were indicated, in accordance with the limited capability of heat to be transformed into potential energy, which the modification of Carnot's principle, previously alluded to, shows, and the unlimited performance of a galvanic engine in raising weights to the full equivalent of chemical force used, which Joule has established.

The transformation of motive power into light, which takes place when work is spent in an extremely concentrated generation of heat, was referred to. It was illustrated by the ignition of platinum wire by means of an electric current driven through it by the chemical force between zinc and oxygen in the galvanic battery; and by the ignition and volatilization of a silver wire by an electric current driven through it by the potential energy laid up in a Leyden battery, when charged by an electrical machine. The luminous heat generated in the last-mentioned case was the complement to a deficiency of heat of friction in the plate-glass and

"On the Heat evolved by Metallic Conductors of Electricity, and in the cells of a battery during Electrolysis."—*Phil. Mag.* Oct. 1841.

"On the Electrical Origin of the Heat of Combustion."—*Phil. Mag.* March 1843.

"On the Heat evolved during the Electrolysis of Water," Proceedings of the Literary and Philosophical Society of Manchester, 1843, Vol. vii. Part 3, Second Series.

"On the Calorific Effects of Magneto-Electricity, and on the Mechanical Value of Heat," communicated to the British Association (Cork), Aug. 1843, and published *Phil. Mag.* Oct. 1843.

"On the Intermittent Character of the Voltaic Current in certain cases of Electrolysis, and on the Intensity of various Voltaic arrangements."—*Phil. Mag.* Feb. 1844.

"On the Mechanical Powers of Electro-Magnetism, Steam, and Horses." By Joule and Scoresby.—*Phil. Mag.* June 1846.

"On the Heat disengaged in Chemical Combination."—*Phil. Mag.* June 1852.

"On the Economical Production of Mechanical Effect from Chemical Forces."—*Phil. Mag.* Jan. 1853.

rubber of the machine, which a perfect determination, and comparison with the amount of work spent in turning the machine, would certainly have detected.

The application of mechanical principles to the mechanical actions of living creatures was pointed out. It appears certain, from the most careful physiological researches, that a living animal has not the power of originating mechanical energy; and that all the work done by a living animal in the course of its life, and all the heat that has been emitted from it, together with the heat that would be obtained by burning the combustible matter which has been lost from its body during its life, and by burning its body after death, make up together an exact equivalent to the heat that would be obtained by burning as much food as it has used during its life, and an amount of fuel that would generate as much heat as its body if burned immediately after birth.

On the other hand, the dynamical energy of luminiferous vibrations was referred to as the mechanical power allotted to plants (not mushrooms or funguses, which can grow in the dark, are nourished by organic food like animals, and like animals absorb oxygen and exhale carbonic acid,) to enable them to draw carbon from carbonic acid, and hydrogen from water.

In conclusion, the sources available to man for the production of mechanical effect were examined and traced to the sun's heat and the rotation of the earth round its axis.

Published speculations* were referred to, by which it is shown to be possible that the motions of the earth and of the heavenly bodies, and the heat of the sun, may all be due to gravitation; or, *that the potential energy of gravitation may be in reality the ultimate created antecedent of all motion, heat, and light at present existing in the universe.**

[W. T.]

* Trans. Roy. Soc. Edinburgh, April 1854 (Professor W. Thomson, "On the Mechanical Energies of the Solar System"). Also British Association Report, Liverpool, Sept. 1854 ("On the Mechanical Antecedents of Motion, Heat, and Light").

Friday, April 4.

SIR HENRY HOLLAND, Bart. M.D. F.R.S. Vice-President,
in the Chair.

HENRY E. ROSCOE, Esq. B.A. Ph.D.

On the Measurement of the Chemical Action of Light.

No attempt has been made, up to the present time, accurately to measure the changes brought about in chemical substances by the action of the solar rays.

The peculiar action of light on chemical bodies was first observed by Scheele on chloride of silver. Since that time the subject of the chemical action of light has attracted a large amount of attention, as the present perfection of the arts of the daguerreotypist and photographer fully testify. Although we possess so many facts concerning the chemical action of light, this branch of science has only as yet arrived at that first or qualitative stage of development, through which every science must pass. The laws which regulate these phenomena are unknown to us, and we possess no means of accurately measuring the amount of the decomposition effected by the light.

The speaker proceeded to describe the results of a series of experiments carried on by him in conjunction with Professor Bunsen, which had for their object :—

1. To determine the laws which regulate the chemical action of light ;
2. To obtain a measure for the chemically active rays.

When aqueous solutions of chlorine, bromine, or iodine, are exposed (under certain conditions) to the direct solar rays they are decomposed, the corresponding hydracid being formed, and the oxygen of the water liberated. The difference between the amounts of free chlorine, bromine, or iodine, contained in the liquid before and after exposure to light, gives the quantity of the substance decomposed during the insolation. Now it was found that this quantity of chlorine, bromine, or iodine, which disappeared, was not proportional to the time of exposure to the light ; in twice the time, for instance, less than twice as much substance was de-

composed. The relation between the amount of light and the amount of decomposition was found in this case not to be a simple one.

This anomalous action may be explained even from a theoretical point of view. Chemical affinity is the resultant of all the forces which come into play during the reaction; hence it is not only the interchanging atoms which influence the result, but also those atoms which, without taking part in the decomposition, surround those actively engaged. The so-called catalytic phenomena show this action in a striking manner. To apply this general principle to the special case before us; we have to begin with pure chlorine water; after the first action of the light, however, hydrochloric acid is formed, hence the composition of the solution is altered, and a different result must be expected. This theoretical conclusion was verified by experiment. Chlorine water, to which 10 per cent. of hydrochloric acid was added, did not suffer any decomposition by an exposure of six hours to the direct sunlight; during which time the same chlorine water, without previous addition of hydrochloric acid, lost nearly all the free chlorine which it contained.*

In order then to obtain a true measure of the action of light on any chemical substance, it is necessary that the body formed by the decomposition should be removed from the sphere of action. This cannot be done with chlorine water; a new sensitive substance was therefore employed.

Equal volumes of chlorine and hydrogen gases when exposed to the direct sun light unite with explosion; in diffuse light, the action proceeds gradually. In presence of water the hydrochloric acid formed by the combination is immediately absorbed, and thus withdrawn from the sphere of action, and the diminution of the volume of the mixed gases arising from this absorption gives an exact measure of the amount of action effected by the light. The diminution in volume of the gas measured by the rise of water in a graduated tube was found to be regular, proving that *when the light is constant the amount of action is directly proportional to the time of exposure.*

The relation between the amount of action and the amount of light was experimentally determined, by allowing known quantities of diffuse light to fall upon the sensitive gas. Experiments thus conducted showed that *the amount of action is directly proportional to the amount or intensity of the light.* These simple relations were observed by Dr. Draper, of New York, in 1843; but his method of experimenting differed essentially from that employed in these researches, and was not susceptible of any very great degree of accuracy. The relation between the amount of action and the mass of the sensitive gas has not as yet been fully determined; experi-

* See Poggendorff's Annalen, xcvi., 373; and Quarterly Journal of Chemical Society, Oct. 1855.

ment has however already shown that the relation is not a simple one.

Many very interesting phenomena were observed in the course of these investigations. When the gas is first exposed to the light no action whatever is observed ; after a short time the absorption slowly begins, and increases until a maximum has been attained, after which it proceeds regularly. This phenomenon of induction probably depends on a peculiar allotropic change which the chlorine must undergo before it is capable of uniting with the hydrogen.

The speaker concluded by expressing his intention of continuing these experiments at Heidelberg, in order exactly to determine the relation which exists between the amount of action and the volume of gas employed ; to investigate the phenomenon of induction ; and to obtain, if possible, an absolute measure for the chemical rays.

[H. E. R.]

Friday, May 16.

SIR CHARLES FELLOWS, Vice-President, in the Chair.

DR. A. W. HOFMANN, F.R.S.

On the Chemical Type: Ammonia.

THE great laws which govern chemical combination, have been mostly recognized and elaborated by the study of *mineral compounds*, the examination of which at a very early period attracted the attention of inquirers. It was only much later:—in fact, at a comparatively recent epoch, that the *vegetable* and the *animal world* were drawn into the circle of chemical observation. The progress made in the study of *vegetable* and *animal* substances was based, in the commencement at least, entirely and exclusively upon the knowledge which chemists possessed of *mineral* bodies. The experience, the ideas, gained in the examination of *mineral* substances, reflected themselves, if I may use this expression in whatever views were brought forward regarding the nature of *vegetable* and *animal* compounds. *Organic Chemistry* was but a reproduction, in another form, of *Mineral*, or *Inorganic Chemistry*.

This aspect, however, of the relative position of the two departments of the science is rapidly changing. The amount of material accumulated by the indomitable perseverance of so many cultivators of Organic Chemistry. (a chaotic and almost inaccessible labyrinth, but a few years ago,) is rapidly assuming shape and order. The study of *organic* bodies has led to the observation of general laws, which could have never been discovered by the examination of *mineral* substances alone, but which begin to react in a most powerful manner upon our ideas regarding the constitution of these very *mineral* substances. The progress of our knowledge of *organic* bodies has opened new points of view, from which the constitution of *mineral* substances appears to us in a brighter light, in a simpler and more intelligible form. In one word, Organic Chemistry is beginning to repay, and I venture to say, with interest, the debt of gratitude which it owes to her elder sister, Mineral Chemistry.

It is my task, this evening, to bring under your notice some especial examples in elucidation of the idea which I have endeavoured to delineate to you. Illustrations of this kind might be taken from widely different departments of the science. In

consequence of special studies and predilections of my own, I have selected as material of illustration a class of substances of which the well-known compound Ammonia is the type.

The four elements—Nitrogen, Phosphorus, Antimony, and Arsenic, although essentially differing in many of their physical properties, exhibit nevertheless an extraordinary similarity in their chemical character, and especially in their combining tendencies. With oxygen these four bodies produce teroxides and pentoxides which, in combination with water, have all decidedly acid properties.

Nitrous Acid . . .	NO_2	Nitric Acid . . .	NO_3
Phosphorous Acid .	PO_3	Phosphoric Acid . .	PO_5
Antimonious Acid .	SbO_3	Antimonic Acid . .	SbO_5
Arsenious Acid . .	AsO_3	Arsenic Acid . . .	AsO_5

The latter acids, moreover, appear to be all tribasic; in phosphoric and arsenic acids the tribasic character is well marked; with antimoniac acid it is less pronounced; and nitric acid is generally considered as a monobasic acid: but the progress of science will, I have no doubt, confirm our suspicion that the nitrogen-acid is likewise of a tribasic character. The chlorides and bromides, corresponding to the oxides of nitrogen, phosphorus, antimony, and arsenic, also exhibit, within certain limits, similar analogies.*

Again, these four elements unite with hydrogen, and the compounds thus produced have a similar composition; they are all terhydrides.

Ammonia	NH_3
Phosphoretted Hydrogen . . .	PH_3
Antimonetted Hydrogen . . .	SbH_3
Arsenetted Hydrogen	AsH_3

So far the analogy appears to be complete. Extraordinary discrepancies, however, are observed in the properties of these hydrogen-compounds, for although they are all gases at the common temperature, although they all possess a marked odour, and are more or less inflammable, we find that ammonia is *soluble* in water, imparting a *strongly alkaline character* to this solution; while the three other compounds, phosphoretted, antimonetted, and arsenetted hydrogens are *insoluble* in water, and *without the slightest alkaline reaction*. Again, ammonia when coming in contact with acids, absorbs these bodies with the greatest avidity, producing a series of well marked, mostly crystalline, compounds, which are called salts of ammonia, or ammoniacal salts, and of which sal ammoniac and sul-

* For the purpose of illustration specimens of *Nitrogen, Phosphorus, Antimony, and Arsenic*, and of their oxides, chlorides, and bromides were upon the table.

phate of ammonia are familiar illustrations. Antimonetted and arsenetted hydrogen, on the other hand, have never been combined with acids; and in the case of phosphoretted hydrogen, only one salt-like compound, the hydriodate of phosphoretted hydrogen is known, which latter certainly presents considerable analogies with the salts of ammonia.

Sulphate of Ammonia	$\text{NH}_3, \text{HSO}_4$
Hydriodate of Ammonia	NH_3, HI
Hydriodate of Phosphoretted Hydrogen	PH_3, HI

The want of similarity observed in the general characters of ammonia and the hydrogen-compounds of phosphorus, antimony, and arsenic, has always been an obstacle in the way of considering the four elements in question as members of the same natural family.*

The modern progress of Organic Chemistry has removed those difficulties.

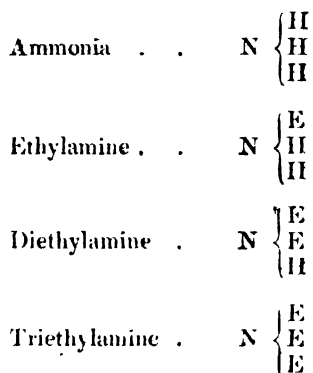
Organic Chemistry deals with compound molecules, consisting of carbon and hydrogen, occasionally associated with nitrogen and oxygen. These compound molecules, often called compound radicals, simulate the deportment and exercise the functions of elementary substances. One of the most familiar illustrations of organic radicals is the radical Ethyl, consisting of four equivalents of carbon, and five of hydrogen, $\text{C}_4\text{H}_5 = \text{E}$, and which chemists assume to exist in *alcohol* and *ether*, the derivation of which from water becomes obvious by a glance at the following formulæ:—

Water	$\left\{ \begin{array}{l} \text{H O} \\ \text{H O} \end{array} \right.$
Alcohol	$\left\{ \begin{array}{l} \text{E O} \\ \text{H O} \end{array} \right.$
Ether	$\left\{ \begin{array}{l} \text{E O} \\ \text{E O} \end{array} \right.$

* The preparation and the principal properties of *ammonia*, *phosphoretted*, *antimonetted*, and *arsenetted* hydrogen were experimentally exhibited. The manufacture of ammoniacal salts was, moreover, illustrated by a series of painted diagrams, and a set of large and beautiful specimens furnished by Messrs. Simpson, Maule, and Nicholson, Kensington. Ammonia was evolved by the action of lime upon sal-ammoniac, phosphoretted hydrogen by that of hydrochloric acid upon phosphide of calcium, antimonetted and arsenetted hydrogen lastly, by introducing antimony- and arsenic-solutions into flasks from which hydrogen was being evolved. Cylinders were filled with the four gases over mercury. The phosphoretted hydrogen evolved proved to be spontaneously inflammable. Antimonetted and arsenetted hydrogen were inflamed by a taper, and ammonia was shown to be capable of combustion by directing the current of the gas through a gas-flame. It was demonstrated that phosphoretted, antimonetted, and arsenetted hydrogens are *not* absorbed by water or even acids, and that they exhibit no alkaline reaction with vegetable colours; whilst ammonia is absorbed by water and acids, and possesses the character of a strong alkali.

Alcohol may be regarded as water, in which *one* equivalent of hydrogen is replaced by ethyl, *ether*, as water, for the *two* hydrogen-equivalents of which ethyl has been substituted. The general characters of these three compounds greatly differ from one another; but some of the fundamental properties of water, its neutral character for instance, are retained in the two substitution-products.*

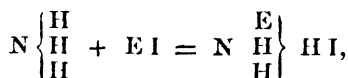
Recent researches have proved that in ammonia likewise the hydrogen-equivalents are replaceable by ethyl. Three new compounds are thus produced, which have received the names *ethylamine*, *diethylamine*, and *triethylamine*: and the composition of which is illustrated by the following formulæ:—



The three ethylated derivatives fully retain the fundamental character of ammonia; they are powerful bases, capable of uniting with the acids, and of forming very definite, well-crystallizing salts. Owing to the diminution of volatility with the progress of ethylation, the ethylated bases appear to be even more powerfully basic than the type itself. This development of basic power, as will be presently seen, deserves especial consideration. The substitution of ethyl for hydrogen presents no difficulty, it may be effected by several methods, one of the commonest processes consisting in the action of iodide of ethyl upon the body to be ethylated. Thus ammonia and iodide of ethyl produce ethylamine and hydriodic acid, which unite and give rise to the formation of hydriodate of ethylamine.†

* Specimens of water, alcohol, and ether placed in juxta-position. Ethyl-gas, obtained by the action of zinc upon the iodide of ethyl in a strong copper digester, exhibited and burnt.

† Ammonia, ethylamine, diethylamine, and triethylamine placed side by side; the alkaline properties of these four substances experimentally demonstrated. Iodide of ethyl, and its action on ammonia exhibited.



In consequence of the ethylated derivatives of ammonia retaining the basic character of the type, and exhibiting it, under certain conditions, even in a higher degree, the question naturally suggested itself, What would be the effect of introducing ethyl into phosphoretted, antimonetted, and arsenetted hydrogen?

The ethylation of these hydrogen-compounds presents difficulties not experienced with the nitrogen-series, and has been accomplished only by roundabout processes. Nor have all the terms, the existence of which theory suggests, as yet been obtained: compounds corresponding to ethylamine and to diethylamine are wanting at present, but the substances which correspond to triethylamine are known.

The following table exhibits the compounds belonging to this group which are known.

<i>Nitrogen-Series.</i>	<i>Phosphorus-Series.</i>	<i>Antimony-Series.</i>	<i>Arsenic-Series.</i>
Ammonia . . . $\text{N} \begin{Bmatrix} \text{H} \\ \text{H} \\ \text{H} \end{Bmatrix}$	Phosphoretted Hydrogen $\text{P} \begin{Bmatrix} \text{H} \\ \text{H} \\ \text{H} \end{Bmatrix}$	Antimonetted Hydrogen $\text{Sb} \begin{Bmatrix} \text{H} \\ \text{H} \\ \text{H} \end{Bmatrix}$	Arsenetted Hydrogen $\text{As} \begin{Bmatrix} \text{H} \\ \text{H} \\ \text{H} \end{Bmatrix}$
Ethylamine . . $\text{N} \begin{Bmatrix} \text{E} \\ \text{H} \\ \text{H} \end{Bmatrix}$	Unknown.	Unknown.	Unknown.
Diethylamine . $\text{N} \begin{Bmatrix} \text{E} \\ \text{E} \\ \text{H} \end{Bmatrix}$	Unknown.	Unknown.	Unknown.
Triethylamine . $\text{N} \begin{Bmatrix} \text{E} \\ \text{E} \\ \text{E} \end{Bmatrix}$	Triethylphosphine . $\text{P} \begin{Bmatrix} \text{E} \\ \text{E} \\ \text{E} \end{Bmatrix}$	Triethylstibine $\text{Sb} \begin{Bmatrix} \text{E} \\ \text{E} \\ \text{E} \end{Bmatrix}$	Triethylarsine $\text{As} \begin{Bmatrix} \text{E} \\ \text{E} \\ \text{E} \end{Bmatrix}$

Now triethylphosphine,* triethylstibine, and triethylarsine are substances exhibiting, although in a less prominent degree, all the fundamental characters of triethylamine, and consequently of ammonia itself. They are well defined and powerful bases, capable of uniting with the acids and of producing a series of remarkable, mostly crystalline salts, in which we find all the properties of the ammoniacal salts. Chemists have thus succeeded in rendering visible to the mental eye, if I may say so, the true nature of phosphoretted, antimonetted, and arsenetted hydrogen. By the conversion of these *mineral* substances into *organic* compounds, by the

* Specimens of triethylphosphine exhibited, and its alkaline characters demonstrated. A small quantity of triethylphosphine was poured into a test-tube filled with oxygen, and placed in hot water, when the phosphorus-compound exploded with great violence. Another portion introduced into chlorine gas, gave rise to a brilliant flash of light, the carbon of the substance being set free. To show the combining power of triethylphosphine, the substance was mixed with iodide of methyl, when a white crystalline compound was immediately formed, with evolution of much heat.

simple process of ethylation, their alkaline disposition, not to use the term character, has been unmistakeably brought to light. The formation of alkaline bodies similar to ammonia by the substitution of ethyl for the hydrogen in phosphoretted, antimonetted, and arsenetted hydrogen, leaves no doubt regarding the analogy of these substances with ammonia, and thus we see that *researches carried out exclusively in the field of ORGANIC CHEMISTRY have lent most valuable assistance in deciding a question of considerable importance regarding the classification of MINERAL SUBSTANCES.* These researches have furnished the last argument which was wanting to prove that nitrogen, phosphorus, antimony, and arsenic form a natural group of elements, the chemical history of which presents analogies not less prominent than those which are observed with the elements chlorine, bromine, and iodine.

The type Ammonia offers another interesting illustration of the influence which the progress of Organic Chemistry exerts upon the Mineral Department of the science, and of the unexpected support which some of the mineral theories have received from the development of our ideas regarding the constitution of organic substances.

Soon after Sir Humphry Davy's immortal discoveries of the alkali-metals, chemists were led by the extraordinary analogy of the salts of these metals with those of ammonia, to assume in the latter a metallic substance similar to potassium and sodium.* Numerous experiments were made to isolate this metallic principle from the ammoniacal salts; and the resources of electricity, which had exhibited such wonderful powers in the hands of Sir H. Davy, were not appealed to without result. The metal itself, it is true, was not isolated; but a compound or alloy was obtained, containing nitrogen and hydrogen, and the metallic character of which was indisputable. If the electric current be passed into a solution of ammonia floating upon a layer of mercury in such a manner, that the positive pole of the battery merely dips into the ammonia, while the negative pole is immersed in the mercury, a very remarkable phenomenon is observed; the mercury begins to swell up and is gradually converted into a mass of buttery consistence, but retaining a perfect *metallic* lustre, while pure nitrogen gas arising from the oxidation of ammonia makes its appearance at the positive pole.† Removed from

* The analogy of the salts of ammonia and those of the fixed alkalies, and especially the isomorphism of the salts of ammonia and potassa, was illustrated by numerous specimens upon the Lecture-table. Ammonia-alum and potassa-alum, sulphate of ammonia and sulphate of potassa, chloride of ammonium and chloride of potassium, &c. &c.

† The ammonium-amalgam was produced on a small scale by the action of the electric current.

the influence of the battery, the altered mercury soon resumes its original appearance, losing at the same time hydrogen and ammonia in the proportion of one equivalent of the former (H) to one of the latter (N H_3). It was therefore argued that the mercury owed the alteration of its properties to its being associated with hydrogen and ammonia, that is with N H_4 ; and since mercury in its combinations never retains any metallic appearance, except in its alloys called amalgams, that is in its combination with metallic substances, chemists considered themselves entitled to attribute *metallic* characters also to the hypothetical association of nitrogen and hydrogen, represented by the formula $\text{N H}_4 = \text{Am}$, for which, forthwith, the name of *Ammonium* was proposed.

By assuming the existence of this hypothetical metal in the salts of ammonia, the nature of these substances, their analogy with the salts of the fixed alkalies, and especially the isomorphism of the salts of ammonia and potassa, became at once intelligible. The following table exhibits the ammoniacal salts (represented firstly, as combinations of *ammonia*, with hydrated acids; and secondly, as *ammonium*-compounds;) in juxtaposition with the corresponding terms of the potassium-series.

	<i>Ammonia-Compounds.</i>	<i>Ammonium-Compounds.</i>		<i>Potassium-Compounds.</i>
Oxide .	$\text{N H}_3, \text{H O}$	$\text{N H}_4 \text{ O}$	or Am O	K O
Chloride	$\text{N H}_3, \text{H Cl}$	$\text{N H}_4 \text{ Cl}$	or Am Cl	K Cl
Sulphate	$\text{N H}_3, \text{H SO}_4$	$\text{N H}_4 \text{ SO}_4$	or Am SO_4	K SO_4
Nitrate .	$\text{N H}_3, \text{H NO}_3$	$\text{N H}_4 \text{ NO}_3$	or Am NO_3	K NO_3

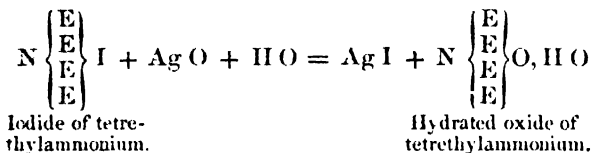
The analogy of chloride, sulphate, and nitrate of ammonium, with the corresponding potassium-compounds, is complete, but the analogy begins to fail when we compare the *oxides* of ammonium and potassium. Oxide of potassium, potash, is a perfectly definite body, the properties of which, especially in its hydrated condition, are well known. On the other hand all attempts to isolate the oxide of ammonium or its hydrate have been hitherto abortive. Liberated from one of the ammonium-compounds it splits at once into ammonia-gas and water, even at the common temperature. The impossibility of producing the oxide of ammonium has been always adduced as an argument against the ammonium-theory.

This difficulty disappears entirely if we examine the deportment of some of the compounds briefly described in the preceding part of this discourse. By again submitting triethylamine, that is ammonia containing three equivalents of ethyl in the place of three

of hydrogen, to the action of iodide of ethyl, a beautiful crystalline compound is obtained, the composition of which is represented by the formula—



that is iodide of ammonium, in which the four hydrogen-equivalents are replaced by a corresponding number of equivalents of ethyl, or *iodide of tetrethylammonium*. If this compound be treated with freshly precipitated oxide of silver, a decomposition takes place, which gives rise to the formation of iodide of silver, separating as a precipitate and of the hydrated ammonium-oxide, corresponding to the above-mentioned iodide or *hydrated oxide of tetrethylammonium*,* which remains in solution. The following equation elucidates this change—



The solution of this compound oxide of ammonium may be evaporated to dryness without decomposition; a crystalline substance is thus obtained of a most powerfully alkaline character, resembling in every respect hydrated potassa itself. A concentrated solution of this substance not only burns the tongue, but acts upon the epidermis which it destroys like potassa or soda. On rubbing the solution between the fingers, the well-known soapy sensation produced by the fixed alkalies under the same circumstances is felt. Moreover the same peculiar odour is perceived. Oxide of tetrethylammonium saponifies the fats without difficulty; beautiful soft soaps are thus obtained, possessing all the properties of ordinary potassa-soap. All the chemical effects produced by potassa or soda, are likewise produced by oxide of tetrethylammonium; and in its deportment with the salts of the metals especially, the compound oxide of ammonium can scarcely be distinguished from the fixed caustic alkalies.

* In three beakers, boiling solutions of iodide of tetrethylammonium (1), of iodide of ammonium (2), and of iodide of potassium (3), were mixed with freshly precipitated oxide of silver. The beakers were then covered with reddened litmus paper and glass plates; after a few moments the paper covering solution (2), was found to have become intensely blue, in consequence of the volatilized ammonia-gas; whilst the papers covering solutions (1) and (3) did not exhibit the slightest alteration, the liberated oxides of tetrethylammonium and of potassium not being volatile. On dipping, however, reddened litmus paper into these two solutions, they were found to be powerfully alkaline.

The formation of oxide of tetrethylammonium is certainly one of the most powerful props which the ammonium-theory could have received, and since this support has been entirely and exclusively furnished by researches performed within the domain of *Organic Chemistry*, it must be admitted that the researches made in this department of the science, and especially the elaboration of the compound derivatives of the type ammonia, begin to react most powerfully and beneficially upon the progress of *Mineral Chemistry*.*

[A. W. H.]

* At the conclusion of the discourse, the ammonium-amalgam was prepared on a large scale by the action of potassium-amalgam upon a solution of chloride of ammonium.

Friday, June 13.

SIR RODERICK I. MURCHISON, G.C.S. F.R.S. Vice-President,
in the Chair.

PROFESSOR FARADAY, D.C.L. F.R.S.

*On M. Petitjean's process for Silvering Glass: some Observations
on divided Gold.*

M. PETITJEAN's process consists essentially in the preparation of a solution containing oxide of silver, ammonia, nitric, and tartaric acids, able to deposit metallic silver either at common or somewhat elevated temperatures; and in the right application of this solution to glass, either in the form of plates or vessels. 1540 grains of nitrate of silver being treated with 955 grains of strong solution of ammonia, and afterwards with 7700 grains of water, yields a solution, to which when clear 170 grains of tartaric acid dissolved in 680 grains of water is to be added, and then 152 cubic inches more of water, with good agitation. When the liquid has settled, the clear part is to be poured off; 152 cubic inches of water to be added to the remaining solid matter, that as much may be dissolved as possible; and the clear fluids to be put together and increased by the further addition of 61 cubic inches of water. This is the silvering solution, No. 1; a second fluid, No. 2, is to be prepared in like manner, with this difference, that the tartaric acid is to be doubled in quantity. The apparatus employed for the silvering of glass plate consists of a cast-iron table box, containing water within, and a set of gas-burners beneath to heat it: the upper surface of the table is planed and set truly horizontal by a level, and covered by a varnished cloth: heat is applied until the temperature is 140° Fah. The glass is well cleaned, first with a cloth; after which a plug of cotton, dipped in the silvering fluid and a little polishing powder, is carefully passed over the surface to be silvered, and when this application is dry it is removed by another plug of cotton, and the plate obtained perfectly clean. The glass is then laid on the table, a portion of the silvering fluid poured on to the surface, and this spread carefully over every part by a cylinder of india-rubber stretched upon wood which has previously been cleaned and wetted with the solution; in this manner a perfect wetting of the surface is obtained, and all air bubbles, &c. are removed. Then more fluid is poured on to the glass until it is covered with a layer about the $\frac{1}{16}$ th of an inch in depth, which easily stands upon it; and in that state its temperature is allowed to rise. In about 10

minutes or more silver begins to deposit on the glass, and in 15 or 20 minutes a uniform opaque coat, having a greyish tint on the upper surface, is deposited. After a certain time the glass employed in the illustration was pushed to the edge of the table, was tilted that the fluid might be poured off, was washed with water, and then was examined. The under surface presented a perfectly brilliant metallic plate of high reflective power, as high as any that silver can attain to; and the coat of silver, though thin, was so strong as to sustain handling, and so firm as to bear polishing on the back to any degree, by rubbing with the hand and polishing powder. The usual course in practice, however, is, when the first stratum of fluid is exhausted, to remove it, and apply a layer of No. 2 solution; and when that has been removed and the glass washed and dried, to cover the back surface with a protective coat of black varnish. When the form of the glass varies, simple expedients are employed; and by their means either concave or convex, or corrugated surfaces are silvered, and bottles and vases are coated internally. It is easy to mend an injury in the silvering of a plate, and two or three cases of repair were performed on the table.

The proposed advantages of the process are,—the production of a perfect reflecting surface; the ability to repair; the mercantile economy of the process (the silver in a square yard of surface is worth 1*s.* 8*d.*); the certainty, simplicity, and quickness of the operation; and, above all, the dismissal of the use of mercury. In theory the principles of the process justify the expectations, and in practice nothing as yet has occurred which is counter to them.

With regard to the second part of the evening's discourse, the speaker said he had been led by certain considerations to seek experimentally for some effect on the rays of light, by bodies which when in small quantities had strong peculiar action upon it, and which also could be divided into plates and particles so thin and minute as to come far within the dimensions of an undulation of light, whilst they still retained more or less of the power they had in mass; and though he had as yet obtained but little new information, he considered it his duty, in some degree, to report progress to the Members of the Royal Institution. The vibrations of light are, for the violet ray 59,570 in an inch, and for the red ray 37,640 in an inch; it is the lateral portion of the vibration of the ether* which is by hypothesis supposed to affect the eye, but the relation of number remains the same. Now a leaf of gold as

* Analogous transverse vibration may easily be obtained on the surface of water or other fluids, by the process described in the *Philosophical Transactions* for 1831, p. 336, &c.

supplied by the mechanician is only $\frac{1}{280,000}$ of an inch in thickness, so that $7\frac{1}{2}$ of these leaves might be placed in the space occupied by a single undulation of the red ray, and 5 in the space occupied by a violet undulation. Gold of this thickness and in this state is transparent, transmitting green light, whilst yellow light is reflected; there is every reason to believe also that some is absorbed, as happens with all ordinary bodies. When gold leaf is laid upon a layer of water on glass, the water may easily be removed, and solutions be substituted for it; in this way a solution of chlorine, or of cyanide of potassium, may be employed to thin the film of gold; and as the latter dissolves the other metals present in the gold, (silver, for instance, which chlorine leaves as a chloride,) it gives a pure result; and by washing away the cyanide, and draining and drying the last remains of water, the film is left attached to the glass: it may be experimented with, though in a state of extreme tenuity. Examined either by the electric lamp, or the solar spectrum, or the microscope, this film was apparently continuous in many parts where its thickness could not be a tenth or twentieth part of the original gold leaf. In these parts gold appeared as a very transparent thing, reflecting yellow light and transmitting green and other rays; it was so thin that it probably did not occupy more than a hundredth part of a vibration of light, and yet there was no peculiar effect produced. The rays of the spectrum were in succession sent through it; a part of all of them was either stopped or turned back, but that which passed through was *unchanged* in its character, whether the gold plate was under ordinary circumstances, or in a very intense magnetic field of force.

When a solution of gold is placed in an atmosphere containing phosphorus vapour the gold is reduced, forming films that may be washed and placed on glass without destroying their state or condition: these vary from extreme thinness to the thickness of gold leaf or more, and have various degrees of reflective and transmissive power; they are of great variety of colour, from grey to green, but they are like the gold leaves in that they do not change the rays of light.

When gold wires are deflagrated by the Leyden discharge upon glass plates, extreme division into particles is effected, and deposits are produced, appearing, by transmitted light, of many varieties of colour, amongst which are ruby, violet, purple, green, and grey tints. By heat many of these are changed so as to transmit chiefly ruby tints, retaining always the reflective character of gold. None of them affect any particular ray selected from the solar spectrum, so as to change its character, otherwise than by reflection and absorption; what is transmitted still remains the same ray. When gold leaf is heated on glass the heat causes its retraction and running together. To common observation the gold leaf disappears, and but little light is then reflected or stopped: but if pressure by a polished agate convex surface be applied to the gold in such

places, reflective power reappears to a greater or smaller degree, and green light is again transmitted. When the gold films by phosphorus have been properly heated, pressure has the same effect with them.

If a piece of clean phosphorus be placed *beneath* a weak gold solution, and especially if the phosphorus be a clear thick film, obtained by the evaporation of a solution of that substance in sulphide of carbon, in the course of a few hours the solution becomes coloured of a ruby tint; and the effect goes on increasing, sometimes for two or three days. At times the liquid appears clear, at other times turbid. As far as Mr. Faraday has proceeded, he believes this fluid to be a mixture of a colourless transparent liquid, with fine particles of gold. By transmitted light, it is of a fine ruby tint; by reflected light, it has more or less of a brown yellow colour. That it is merely a diffusion of fine particles is shown by two results; the first is, that the fluid being left long enough the particles settle to the bottom: the second is, that whilst it is coloured or turbid, if a cone of the sun's rays (or that from a lamp or candle in a dark room) be thrown across the fluid by a lens, the particles are illuminated, reflect yellow light, and become visible, not as independent particles, but as a cloud. Sometimes a liquid which has deposited much of its gold, remains of a faint ruby tint, and to the ordinary observation, transparent; but when illuminated by a cone of rays the suspended particles show their presence by the opalescence, which is the result of their united action. The settling particles, if in a flask, appear at the bottom, like a lens of deep coloured fluid, opaque at the middle, but deep ruby at the edges; when agitated they may be again diffused through the liquid. These particles tend to aggregate into larger particles, and produce other effects of colour. It is found that boiling gives a certain degree of permanence to the ruby state. Many saline and other substances affect this ruby fluid: thus, a few drops of solution of common salt being added, the whole gradually becomes of a violet colour; still the particles are only in suspension, and when illuminated by a lens are a golden yellow by reflected light: they separate now much more rapidly and perfectly by deposition from the fluid than before. Some specimens, however, of the fluid, of a weak purple or violet colour, remain for months without any appearance of settling, so that the particles must be exceedingly divided; still the rays of the sun or even of a candle in a dark room, when collected by a lens, will manifest their presence. The highest powers of the microscope have not as yet rendered visible either the ruby or the violet particles in any of these fluids.

Glass is occasionally coloured of a ruby tint by gold; such glass, when examined by a ray of light and a lens, gives the opalescent effect described above, which indicates the existence of separate particles; at least such has been the case with all the specimens Mr. Faraday has examined. It becomes a question whether the con-

stitution of the glass and the ruby fluids described is not, as regards colour, alike. At present, he believes they are ; but whether the gold is in the state of pure metal, or of a compound, he has yet to decide. It would be a point of considerable optical importance if they should prove to be metallic gold ; from the effects presented when gold wires are deflagrated by the Leyden discharge over glass, quartz, mica, and vellum, and the deposits subjected to heat, pressure, &c., he inclines to believe they are pure metal.

[M. F.]

Friday, February 6.

SIR BENJAMIN COLLINS BRODIE, Bart. D.C.L. F.R.S.
Vice-President, in the Chair.

JOHN HALL GLADSTONE, Ph.D. F.R.S. M.R.I.

On Chromatic Phenomena exhibited by Transmitted Light.

THE origin of colour was first illustrated by some elementary remarks and experiments. It was laid down as a fundamental principle, that the colour of an object depends on its reflecting or transmitting those rays of light which are capable of producing the sensation of the said colour. The objection that a rose is red not only when viewed by red light, but when seen in colourless daylight, was answered by showing that a beam of colourless light from the electric lamp really consisted of very many coloured rays, and was resolvable by a prism into a red, orange, yellow, green, blue, indigo, and violet light. This, when received on a white screen, showed a brilliantly coloured spectrum, and brightly tinted objects appeared of their ordinary hue only when illuminated by the ray of the same colour. It was explained that the electric light closely resembles that of the sun, but that the light of the great luminary is deficient in certain rays, so that a prismatic spectrum formed by daylight is traversed by very thin dark lines, which have been mapped and designated A, B, C, D, &c. Most artificial lights contain certain coloured rays in excess, hence objects illuminated by them exhibit that colour more prominently than by daylight. The soda flame, for instance, consists almost wholly of certain yellow rays which are wanting in the sun's light, coinciding in refrangibility (as Mr. Crookes has shown) with the dark line D; hence red or blue objects illuminated by it appear black, and nothing is reflected from those which do appear luminous excepting a ghastly yellow.

Leaving reflected, and turning to transmitted light, it was seen that pieces of coloured glass, interposed in the beam of light from the electric lamp, stopped certain rays, while they allowed others to pass through; thus a red glass cut off all the blue end of the spectrum, while a smalt-blue glass divided the red end into several

luminous bands alternating with dark spaces. The same was true of coloured liquids, a solution of sulphate of indigo absorbing the orange and yellow rays, and giving a spectrum consisting of a red ray separated by a broad black space from the green, blue, and violet. An oxy-hydrogen line light, covered successively by red, yellow, and blue bell-glasses, produced the same effect on the coloured diagrams and other objects around, as if the source of light had been alternately red, yellow, and blue; and the opacity of these glasses to certain rays, and their transparency to others, was further illustrated by burning spirit lamps, the wicks of which had been previously sprinkled with salt, under yellow and blue bell-glasses. The yellow glass appeared perfectly transparent to the light which it covered, but the blue did not suffer the least yellow to pass; indeed, the soda-flame under it seemed of a pale violet tipped with green. It was explained that cobalt (to which the colour of the blue glass is owing) absorbs all those rays which are about the dark line D of the spectrum, although it suffers those a little less or a little more refrangible to pass freely; hence a considerable portion of the yellow light of the sun will penetrate such blue glass, but the yellow of the soda-flame is absolutely stopped. As a converse experiment, sulphuret of carbon lamps were ignited under the yellow and blue glasses; when the blue cover appeared almost transparent and colourless, while the yellow was opaque to the blue light, transmitting only some greenish rays.

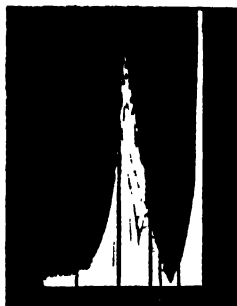
If white light be transmitted through two or more media successively, each of which has a different absorbing effect upon it, very unexpected results may be frequently obtained. This is true of combinations of coloured glasses, or of coloured liquids. A red solution of meconate of iron, for instance, appears black when seen through the blue solution of an ammoniacal copper salt. If a vessel, filled with the blue alcoholic solution of a cobalt salt, be immersed in a pale yellow solution of chromic acid, it appears to contain a deep red liquid. Green nitrate of chromium also becomes red, when looked at through the same yellow solution. Similarly when two coloured compounds are mixed together, which are incapable of entering into chemical combination, an unexpected colour will frequently result; thus, on adding a little blue sulphindigotate of potash to a solution of yellow chromate of potash, the result was green, but on adding a larger amount of the blue salt it changed to red. There was here no chemical change; yet how naturally might a chemist have received the unlooked-for colour as evidence of a new compound!

This experiment introduced the subject of dichromatism. A thin stratum of even a highly coloured liquid is almost destitute of colour; thus the bubbles formed on shaking acetate of iron, or more familiarly, port-wine, or porter, appear white. That the colour of a solution changes in intensity, becoming paler when diluted, and deeper when concentrated, is known to all. This is

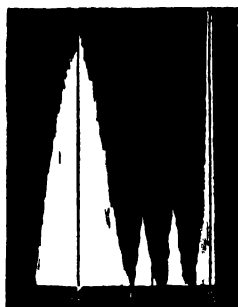
the general rule; yet a solution of yellow chromate of potash appeared scarcely any paler when diluted with perhaps twenty times its bulk of water. Sometimes also a complete change of colour takes place; thus acetate of chromium, which was red, became green when considerably diluted with pure water: a few drops of cochineal, stirred up in a tall champagne glass filled with water, imparted a red tint to the upper wide portion, and a lavender tint to the lower and narrow portion. A neutral solution of litmus is blue, alkalies render this (as is well known) still more blue, boracic or carbonic acid changes it to a wine red, and other acids to a bright red: yet slightly acid litmus was exhibited of a pale purple hue, and alkaline litmus of a deep red colour. All these phenomena were stated to be dependent, not on any chemical action exerted by the water, but on the quantity of the colouring substance traversed by the light in its passage to the eye; the same solution appearing of different colours according to the thickness seen through, and a deep stratum of a dilute liquid having the same tint as a shallow stratum of the same liquid when strong. The speaker added, that this phenomenon had been fully described and explained by Sir John Herschel; who termed it *Dichromatism*, but that fresh instances of it were being constantly observed; indeed, after investigating some cases of it last summer, he had, during a tour on the continent, noticed a fruit sauce which constantly appeared at the hotel dinners in Bavaria and other parts of Germany, and was beautifully dichromatic, red and blue, with every intervening shade of purple. By this character he had traced its composition, and found the colour was due to the deep red cherries which were very abundant at that season. He had noticed the phenomenon likewise in some specimens of the ordinary wine, in essence of lavender, in the syrup of green-gage tart, as well as in some pure chemical substances, such as red prussiate of potash, meconate of iron, purple comenamate of iron, citrate of iron, sulphindigotic acid, and permanganate of potash.

The prism reveals the origin of all these chromatic phenomena. It shows that the different rays of the spectrum are capable of penetrating different distances into a coloured medium. Thus, if port wine be placed in a wedge-shaped glass vessel, and this interposed in the refracted rays in such a position that each coloured ray can fall upon the different thicknesses of the liquid, it will be found that all the rays of the spectrum can penetrate a thin stratum, but that as the liquid increases in depth all are absorbed except the least refrangible red. Hence the thin film of a bubble of port is colourless. If yellow chromate of potash be examined in a similar manner, it is found to cut off the blue and violet rays at once, and to transmit the less refrangible half of the spectrum with equal freeness whether the stratum be thin or thick. Hence it is that dilution scarcely diminishes the colour of this dissolved salt. If a wedge of cobalt-blue glass (which is dichromatic) be inter-

posed across the spectrum, either of the sun, or of the electric light, a remarkable configuration is observed, which shows that the luminous bands of orange-red, of pale green, of orange, of yellow, of blue, and of violet, are absorbed at different distances in the order above given, while the extreme red penetrates any thickness with almost undiminished brightness. Acetate of chromium, placed in a hollow glass wedge, was seen to transmit the red, orange, green, blue, and violet rays through a thin stratum; yellow was absorbed at once, violet very quickly, while the maxima of luminosity were in the extreme red, and about the junction of green and blue, which in the solar spectrum is marked by the dark line F. These blue and green rays, however, which are transmitted in such quantity at first that the solution appears green to the unaided eye, are gradually absorbed, while the red ray continues to penetrate the dense solution, which of course assumes a red colour. A solution of litmus was seen to transmit the red, green, and blue rays freely, the maximum of absorption taking place between the fixed lines C and D of the solar spectrum: the addition of an alkali made little alteration beyond facilitating the transmission of the blue ray; while an acid diminished though it did not entirely retard it, causing the admission at the same time of the orange ray, and shifting the maximum of absorption to between *b* and F. As the red ray passes apparently unchanged through a great thickness of any of these solutions, neutral, alkaline, and acid litmus, all appear red if seen in sufficient quantity; indeed, paradoxical as it may sound, alkaline litmus is then of a purer red than acid litmus, since the latter transmits some orange light as well. The various appearances of the prismatic spectrum, as seen through these liquids in wedge-shaped vessels may be easily copied by a draughtsman; and, in fact, coloured diagrams were displayed, representing the prismatic images given by most of the above-mentioned substances. Some of these presented very characteristic forms: thus, cochineal showed two maxima of transmissibility, about B and G, penetrating far into the liquid, and two others in the green space, which, however, were speedily absorbed. Tincture of lavender, too, gave a spectrum marked by absorption bands, which seemed to coincide with the lines *b*, F, and G of the solar spectrum, though

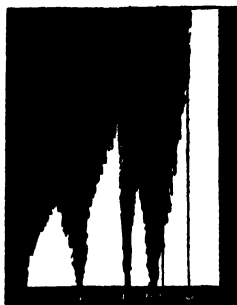
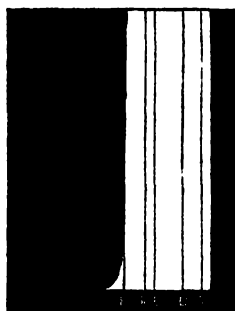


Salt of Chromium.



Cochineal.

broader than these : the violet and green were more quickly absorbed than the blue rays, and these more quickly than the orange and red. The speaker observed, that all the solutions of chromium salts which he had examined, whether green, blue, or red, gave a prismatic image of the same form,—that described above,—the only perceptible difference being in the relative luminosity of the different colours : thus, on examining the green and blue modifications of nitrate of chromium in solutions of the same strength, the green in the first case appeared brighter than the blue, and penetrated to a somewhat greater distance, while in the second case it was the blue that had the advantage in luminosity ; but the general configuration of the prismatic image was identical in the two modifications so different in appearance to the unaided eye. This was not the only instance in which the prism had revealed a wider application to the general rule, that a particular base or acid has the same, or very nearly the same effect, upon the rays of light, with whatever it may be combined. When two colouring substances combine, each continues to exert its proper influence on the various rays ; thus, acid chromate of copper is yellowish green, because the chromic acid absorbs the blue and violet rays, and the copper the red ray, and thus orange, yellow, and green are alone transmitted. The diagrams also explained the production of first green, and afterwards red, on the admixture of sulphindigotate and

*Lavender.**Sulphindigotate of Potash.**Chromate of Potash.*

chromate of potash. Sulphindigotic acid and its salts admit the extreme red freely, but absorb the orange at once, the yellow very speedily, the green not so soon, and admit the blue and violet to a considerable distance : these last, however, are completely absorbed by chromic acid and the chromates ; thus, a little red and much

green pass through a thin stratum of the mixed salts, while red alone is transmitted by a thick stratum.

Sir David Brewster observed that some coloured media caused a ray of a certain angle of refraction to appear of a different colour to that which it exhibited in the normal spectrum; and, mainly on these observations, he founded his remarkable theory that the prismatic spectrum consists of three superimposed spectra of the same length, one red, another yellow, and the third blue, which are coincident in position, but have their maxima of luminosity at different places. Some eminent philosophers of our own and other lands have denied not merely the conclusions, but even the observations of Brewster. Dr. Gladstone, however, could add his fullest testimony to the truth of the statement, that absorbent media frequently produce an apparent change of colour in a transmitted ray; and that not merely when a slit in the window-shutter is viewed by a prism through the interposed medium, but also when the altered prismatic spectrum is thrown upon a white screen. He had tried the latter experiment by means of light derived from the sun, from the electric lamp of the Royal Institution, and from the oxy-gas lime lamp of Mr. Highley, with the kind assistance of that gentleman, and always with a similar result. The large bell-glasses used during this discourse had been originally employed by Dr. Gladstone for experiments on the growth of plants, and he had then carefully examined the light transmitted through them. Through the blue glass he saw first a band of pure red light, then a dark space, then another luminous band which appeared to him like no colour of the spectrum, rather russet perhaps; his assistant called it "dirty chocolate;" a lady, who happened to come into the laboratory, unhesitatingly pronounced it "orange;" he was struck with this, as it certainly corresponded in position with the orange ray, though he did not know at that time, what has been frequently observed, that women are generally far more accurate in their appreciation of colours than men are. Accordingly he described the luminous band in his note-book as "orange, very bright, but unlike normal colour." Subsequently, he had found that Brewster called this second red ray in smalt blue glass "orange red;" but Herschel pronounced it "pure red;" while Helmholtz, isolating it from surrounding light, resolved it into its proper orange. Quite recently, on examining the prismatic spectrum thrown on a screen after traversing the same kind of glass, one scientific friend had called the second luminous ray "green," and another had designated it "brown," though on reconsideration each independently thought it had rather a reddish tint.

Thus Brewster's observation that a ray after transmission through certain absorbent media appears of a different colour to what it did before, is a truth. Yet the very fact that this colour seems so different to different eyes, and indeed to the same eye at different times, indicates that the phenomenon has a subjective rather than an ob-

jective origin. Difficulties of another character have also been urged against Brewster's deduction, by Helmholtz and others, and may be drawn from Maxwell's experiments. It is certain that changes in the apparent colour of a particular ray may arise from other causes than the absorption of one kind of light, while another kind having the same angle of refraction is transmitted. Of these may be enumerated :—First, an actual change of refrangibility, as in the cases of "fluorescence," so fully investigated by Professor Stokes. Secondly, a difference in the impression on the sense, arising from change of intensity. Thus blue, if very luminous, inclines to white, if faintly luminous to violet; and so the fore-mentioned notebook designates the faint rays about F that were transmitted by the red bell-glass, "li'ac," and the speaker had observed the blue in the prismatic spectra given by ammonio-sulphate of nickel, and by tincture of lavender, gradually shading off into violet as the light passed through deeper strata of liquid. The yellow of the solar spectrum appears to occupy a considerable space, if the sun be bright, but if diffuse daylight be examined, that space appears orange and green, while the yellow is perhaps confined to a very luminous line a little beyond D. It is not to be wondered at therefore that the green in the spectra of port wine and of citrate of iron, appears to invade the space usually occupied by the yellow, and the orange yellow. Yet in such cases the impression on different eyes may be very different; thus, in rehearsing the experiments with the electric light at the Royal Institution, Dr. Gladstone had seen the bright space beyond D transmitted by blue glass of a decidedly green tint; but Mr. Anderson had unhesitatingly called it yellow, its proper colour. This difference of sensation, arising from difference of intensity, was illustrated by the "Cercles chromatiques" of M. Chevreul, the first of which represents the bright colours of the spectrum, in which that called "*Jaune*" is certainly a beautiful yellow; but the succeeding circles represent the same, reduced by the admixture of various percentages of black, and in them the "*Jaune*" becomes *green*, and so likewise does the "*Orange*," where a very large proportion of black has been added. A revolving disk, coloured black, on which had been fastened a segment of bright yellow paper, appeared uniformly green when set in rapid motion. Again, on one of Maxwell's colour tops was fixed an outer circle of red, and an inner one, partly black and partly orange; when the top was spun the inner circle appeared green. Thirdly, contrast will frequently change the apparent colour of a particular ray. The result in the last experiment was partially due to this cause, the outer circle of bright red facilitating the sensation of its complementary colour green. Thus the dim light between D and E in the spectrum of ammonio-sulphate of nickel, with bright orange on one side and green on the other, assumes a very indefinite tint. The very remarkable prismatic image given by a solution of permanganate of potash in

the wedge-shaped vessel was exhibited, and it was seen that the orange band became very faint when the solution was deep, and in contrast with the neighbouring brilliant red appeared sometimes green, but more generally violet. Much, in this case also, was found to depend on the eye of the observer; but that a violet sensation might be produced from orange under such circumstances had been proved by the speaker, who in repeating one of Dr. Tyndall's experiments—that of looking at the daylight through a red glass on which a vermilion wafer was fastened—had frequently seen the wafer assume a violet tint. He believed that these three causes were sufficient to account for all the apparent changes of colour produced in a ray by absorbent media.

[J. H. G.]

Friday, February 27.

H.R.H. PRINCE ALBERT, K.G. D.C.L. F.R.S. Vice-Patron,
in the Chair.

PROFESSOR FARADAY, D.C.L. F.R.S.

On the Conservation of Force.

VARIOUS circumstances induce me at the present moment, to put forth a consideration regarding the conservation of force. I do not suppose that I can utter any truth respecting it, that has not already presented itself to the high and piercing intellects which move within the exalted regions of science; but the course of my own investigations and views makes me think, that the consideration may be of service to those persevering labourers (amongst whom I endeavour to class myself), who, occupied in the comparison of physical ideas with fundamental principles, and continually sustaining and aiding themselves by experiment and observation, delight to labour for the advance of natural knowledge, and strive to follow it into undiscovered regions.

There is no question which lies closer to the root of all physical knowledge, than that which inquires whether force can be destroyed or not. The progress of the strict science of modern times has tended more and more to produce the conviction that "force can neither be created nor destroyed;" and to render daily more manifest the value of the knowledge of that truth in experimental research. To admit, indeed, that force may be destructible or can altogether disappear, would be to admit that matter could be uncreated; for we know matter only by its forces; and though one of these is most commonly referred to, namely gravity, to prove its presence, it is not because gravity has any pretension, or any exemption, amongst the forms of force as regards the principle of *conservation*; but simply that being, as far as we perceive, inconvertible in its nature and unchangeable in its manifestation, it offers an unchanging test of the matter which we recognize by it.

Agreeing with those who admit the conservation of force to be a principle in physics, as large and sure as that of the indestructibility of matter, or the invariability of gravity, I think that no particular idea of force has a right to unlimited or unqualified acceptance, that does not include *assent* to it; and also, to *definite amount* and *definite disposition of the force*, either in one effect or another, for these are necessary consequences: therefore, I urge, that the conservation of force ought to be admitted as a physical

principle in all our hypotheses, whether partial or general, regarding the actions of matter. I have had doubts in my own mind whether the considerations I am about to advance are not rather metaphysical than physical. I am unable to define what is metaphysical in physical science; and am exceedingly adverse to the easy and unconsidered admission of one supposition upon another, suggested as they often are by very imperfect induction from a small number of facts, or by a very imperfect observation of the facts themselves: but, on the other hand, I think the philosopher may be bold in his application of principles which have been developed by close inquiry, have stood through much investigation, and continually increase in force. For instance, *time* is growing up daily into importance as an element in the exercise of force. The earth moves in its orbit in time; the crust of the earth moves in time; light moves in time; an electro-magnet requires time for its charge by an electric current: to inquire, therefore, whether power, acting either at sensible or insensible distances, always acts in *time*, is not to be metaphysical; if it acts in time and across space, it must act by physical lines of force; and our view of the nature of the force may be affected to the extremest degree by the conclusions, which experiment and observation on time may supply: being, perhaps, finally determinable only by them. To inquire after the possible time in which gravitating, magnetic, or electric force is exerted, is no more metaphysical than to mark the times of the hands of a clock in their progress; or that of the temple of Serapis in its ascents and descents; or the periods of the occultations of Jupiter's satellites; or that in which the light from them comes to the earth. Again, in some of the known cases of action in time, something happens whilst the *time* is passing which did not happen before, and does not continue after: it is, therefore, not metaphysical to expect an effect in *every* case, or to endeavour to discover its existence and determine its nature. So in regard to the principle of the conservation of force; I do not think that to admit it, and its consequences, whatever they may be, is to be metaphysical: on the contrary, if that word have any application to physics, then I think that any hypothesis, whether of heat, or electricity, or gravitation, or any other form of force, which either wittingly or unwittingly dispenses with the principle of conservation, is more liable to the charge, than those which, by including it, become so far more strict and precise.

Supposing that the truth of the principle of the conservation of force is assented to, I come to *its uses*. No hypothesis should be admitted nor any assertion of a fact credited, that denies the principle. No view should be inconsistent or incompatible with it. Many of our hypotheses in the present state of science may not comprehend it, and may be unable to suggest its consequences; but none should oppose or contradict it.

If the principle be admitted, we perceive at once, that a theory

or definition, though it may not contradict the principle cannot be accepted as sufficient or complete unless the former be contained in it; that however well or perfectly the definition may include and represent the state of things commonly considered under it, that state or result is only partial, and must not be accepted as exhausting the power or being the full equivalent, and therefore cannot be considered as representing its *whole nature*; that, indeed, it may express only a very small part of the whole, only a residual phenomenon, and hence give us but little indication of the full natural truth. Allowing the principle its force, we ought, in every hypothesis, either to account for its consequences by saying what the changes are when force of a given kind apparently disappears, as when ice thaws, or else should leave space for the idea of the conversion. If any hypothesis, more or less trustworthy on other accounts, is insufficient in expressing it or incompatible with it, the place of deficiency or opposition should be marked as the most important for examination; for there lies the hope of a discovery of new laws or a new condition of force. The deficiency should never be accepted as satisfactory, but be remembered and used as a stimulant to further inquiry; for conversions of force may here be hoped for. Suppositions may be accepted for the time, provided they are not in contradiction with the principle. Even an increased or diminished capacity is better than nothing at all; because such a supposition, if made, must be consistent with the nature of the original hypothesis, and may, therefore, by the application of experiment, be converted into a further test of probable truth. The case of a force simply removed or suspended, without a transferred exertion in some other direction, appears to me to be absolutely impossible.

If the principle be accepted as true, we have a right to pursue it to its consequences, no matter what they may be. It is, indeed, a duty to do so. A theory may be perfection, as far as it goes, but a consideration going beyond it, is not for that reason to be shut out. We might as well accept our limited horizon as the limits of the world. No magnitude, either of the phenomena or of the results to be dealt with, should stop our exertions to ascertain, by the use of the principle, that something remains to be discovered, and to trace in what direction that discovery may lie.

I will endeavour to illustrate some of the points which have been urged, by reference, in the first instance, to a case of power, which has long had great attractions for me, because of its extreme simplicity, its promising nature, its universal presence, and its invariability under like circumstances; on which, though I have experimented* and as yet failed, I think experiment would be well bestowed: I mean the force of gravitation. I believe I represent the received idea of the gravitating force aright, in saying, that it is

* Philosophical Transactions, 1851, p. 1.

a simple attractive force exerted between any two or all the particles or masses of matter, at every sensible distance, but with a strength varying inversely as the square of the distance. The usual idea of the force implies *direct* action at a distance; and such a view appears to present little difficulty except to Newton, and a few, including myself, who in that respect, may be of like mind with him. *

This idea of gravity appears to me to ignore entirely the principle of the conservation of force; and by the terms of its definition, if taken in an absolute sense "*varying* inversely as the square of the distance" to be in direct opposition to it; and it becomes my duty, now, to point out where this contradiction occurs, and to use it in illustration of the principle of conservation. Assume two particles of matter A and B, in free space, and a force in each or in both by which they gravitate towards each other, the force being unalterable for an unchanging distance, but varying inversely as the square of the distance when the latter varies. Then, at the distance of 10 the force may be estimated as 1; whilst at the distance of 1, *i.e.* one-tenth of the former, the force will be 100: and if we suppose an elastic spring to be introduced between the two as a measure of the attractive force, the power compressing it will be a hundred times as much in the latter case as in the former. But from whence can this enormous increase of the power come? If we say that it is the character of this force, and content ourselves with that as a sufficient answer, then it appears to me, we admit a *creation* of power, and that to an enormous amount; yet by a change of condition, so small and simple, as to fail in leading the least instructed mind to think that it can be a sufficient cause:—we should admit a result which would equal the highest act our minds can appreciate of the working of infinite power upon matter; we should let loose the highest law in physical science which our faculties permit us to perceive, namely, the *conservation of force*. Suppose the two particles A and B removed back to the greater distance of 10, then the force of attraction would be only a hundredth part of that they previously possessed; this, according to the statement that the force varies inversely as the square of the distance would double the strangeness of the above results; it would be an *annihilation* of force; an effect equal in its infinity and its consequences with *creation*, and only within the power of Him who has created.

We have a right to view gravitation under every form that either its definition or its effects can suggest to the mind; it is our privilege to do so with every force in nature; and it is only by so doing, that we have succeeded, to a large extent, in relating the various forms of power, so as to derive one from another, and thereby obtain confirmatory evidence of the great principle of the conservation of force. Then let us consider the two particles A and

* See Note, p. 358.

B as attracting each other by the force of gravitation, under another view. According to the definition, the force depends upon both particles, and if the particle A or B were by itself, it could not gravitate, *i.e.* it could have no attraction, no *force* of gravity. Supposing A to exist in that isolated state and without gravitating force, and then B placed in relation to it, gravitation comes on, as is supposed, on the part of both. Now, without trying to imagine *how* B, which had no gravitating force, can raise up gravitating force in A; and how A, equally without force beforehand can raise up force in B, still, to imagine it as a fact done, is to admit a creation of force in both particles; and so to bring ourselves within the impossible consequences which have already been referred to.

It may be said we cannot have an idea of one particle by itself, and so the reasoning fails. For my part I can comprehend a particle by itself just as easily as many particles; and though I cannot conceive the relation of a lone particle to gravitation, according to the limited view which is at present taken of that force, I can conceive its relation to something which causes gravitation, and with which, whether the particle is alone, or one of a universe of other particles, it is always related. But the reasoning upon a lone particle does not fail; for as the particles can be separated, we can easily conceive of the particle B being removed to an infinite distance from A, and then the power in A will be infinitely diminished. Such removal of B will be as if it were annihilated in regard to A, and the force in A will be annihilated at the same time: so that the case of a lone particle and that where different distances only are considered become one, being identical with each other in their consequences. And as removal of B to an infinite distance is as regards A annihilation of B, so removal to the smallest degree is, in principle, the same thing with displacement through infinite space: the smallest increase in distance involves annihilation of power; the annihilation of the second particle, so as to have A alone, involves no other consequence in relation to gravity; there is difference in degree, but no difference in the character of the result.

It seems hardly necessary to observe, that the same line of thought grows up in the mind if we consider the mutual gravitating action of one particle and many. The particle A will attract the particle B at the distance of a mile with a certain degree of force; it will attract a particle C at the same distance of a mile with a power equal to that by which it attracts B; if myriads of like particles be placed at the given distance of a mile, A will attract each with equal force; and if other particles be accumulated round it, within and without the sphere of two miles diameter, it will attract them all with a force varying inversely with the square of the distance. How are we to conceive of this force growing up in A to a million fold or more? and if the surrounding particles be then removed, of its diminution in an equal degree? Or, how are

we to look upon the power raised up in all these outer particles by the action of A on them, or by their action one on another, without admitting, according to the limited definition of gravitation, the facile generation and annihilation of force?

The assumption which we make for the time with regard to the nature of a power (as gravity, heat, &c.), and the form of words in which we express it, *i.e.* its definition, should be consistent with the fundamental principles of force generally. The conservation of force is a fundamental principle; hence the assumption with regard to a particular form of force, ought to imply what becomes of the force when its action is *increased* or *diminished*, or its *direction changed*; or else the assumption should admit that it is deficient on that point, being only half competent to represent the force; and, in any case, should not be opposed to the principle of conservation. The usual definition of gravity as *an attractive force between the particles of matter VARYING inversely as the square of the distance*, whilst it stands as a full definition of the power, is inconsistent with the principle of the conservation of force. If we accept the principle, such a definition must be an imperfect account of the whole of the force, and is probably only a description of one exercise of that power, whatever the nature of the force itself may be. If the definition be accepted as tacitly including the conservation of force, then it ought to admit, that consequences must occur during the suspended or diminished degree of its power as gravitation, equal in importance to the power suspended or hidden; being in fact equivalent to that diminution. It ought also to admit, that it is incompetent to suggest or deal with any of the consequences of that changed part or condition of the force, and cannot tell whether they depend on, or are related to, conditions *external* or *internal* to the gravitating particle; and, as it appears to me, can say neither yes nor no to any of the arguments or probabilities belonging to the subject.

If the definition *denies* the occurrence of such contingent results, it seems to me to be unphilosophical; if it simply *ignores* them, I think it is imperfect and insufficient; if it *admits* these things, or any part of them, then it prepares the natural philosopher to look for effects and conditions as yet unknown, and is open to any degree of development of the consequences and relations of power: by denying, it opposes a dogmatic barrier to improvement; by ignoring, it becomes in many respects an inert thing, often much in the way; by admitting, it rises to the dignity of a stimulus to investigation, a pilot to human science.

The principle of the conservation of force would lead us to assume, that when A and B attract each other less because of increasing distance, then some other exertion of power, either within or without them, is proportionately growing up; and again, that when their distance is diminished, as from 10 to 1, the power of attraction, now increased a hundred-fold, has been produced out of

some other form of power which has been equivalently reduced. This enlarged assumption of the nature of gravity is not more metaphysical than the half assumption; and is, I believe, more philosophical, and more in accordance with all physical considerations. The half assumption is, in my view of the matter, more dogmatic and irrational than the whole, because it leaves it to be understood, that power can be created and destroyed almost at pleasure.

When the equivalents of the various forms of force, as far as they are known, are considered, their differences appear very great; thus, a grain of water is known to have electric relations equivalent to a very powerful flash of lightning. It may therefore be supposed that a very large apparent amount of the force causing the phenomena of gravitation, may be the equivalent of a very small change in some unknown condition of the bodies, whose attraction is varying by change of distance. For my own part, many considerations urge my mind toward the idea of a cause of gravity, which is not resident in the particles of matter merely, but constantly in them, and all space. I have already put forth considerations regarding gravity which partake of this idea,* and it seems to have been unhesitatingly accepted by Newton.†

There is one wonderful condition of matter, perhaps its only true indication, namely *inertia*; but in relation to the ordinary definition of gravity, it only adds to the difficulty. For if we consider two particles of matter at a certain distance apart, attracting each other under the power of gravity and free to approach, they will approach; and when at only half the distance each will have had stored up in it, because of its *inertia*, a certain amount of mechanical force. This must be due to the force exerted, and, if the conservation principle be true, must have consumed an equivalent proportion of the cause of attraction; and yet, according to the definition of gravity, the attractive force is not diminished thereby, but increased four-fold, the force growing up within itself the more rapidly, the more it is occupied in producing other force. On the other hand, if mechanical force from without be used to separate the particles to twice their distance, this force is not stored up in momentum or by inertia, but disappears: and three-fourths of the attractive force at the first distance disappears with it: How can this be?

* Proceedings of the Royal Institution, 1855, Vol. II., p. 10, &c.

† "That gravity should be innate, inherent, and essential to matter, so that one body may act upon another at a distance, through a *vacuum*, without the mediation of any thing else, by and through which their action and force may be conveyed from one to another, is to me so great an absurdity that I believe no man who has in philosophical matters a competent faculty of thinking, can ever fall into it. Gravity must be caused by an agent, acting constantly according to certain laws; but whether this agent be material or immaterial I have left to the consideration of my readers."—See *Newton's Third Letter to Bentley*.

We know not the physical condition or action from which *inertia* results; but inertia is always a pure case of the conservation of force. It has a strict relation to gravity, as appears by the proportionate amount of force which gravity can communicate to the inert body; but it appears to have the same strict relation to other forces acting at a distance as those of magnetism or electricity, when they are so applied by the tangential balance as to act independent of the gravitating force. It has the like strict relation to force communicated by impact, pull, or in any other way. It enables a body to take up and conserve a given amount of force until that force is transferred to other bodies, or changed into an equivalent of some other form; that is all that we perceive in it: and we cannot find a more striking instance amongst natural, or possible, phenomena of the necessity of the conservation of force as a law of nature; or one more in contrast with the assumed variable condition of the gravitating force supposed to reside in the particles of matter.

Even gravity itself furnishes the strictest proof of the conservation of force in this, that its power is unchangeable for the same distance; and is by that in striking contrast with the variation which we assume in regard to the *cause of gravity*, to account for the *results* at different distances.

It will not be imagined for a moment that I am opposed to what may be called the *law of gravitating action*, that is, the law by which all the known effects of gravity are governed; what I am considering, is the definition of the *force* of gravitation. That the result of one exercise of a power may be inversely as the square of the distance, I believe and admit; and I know that it is so in the case of gravity, and has been verified to an extent that could hardly have been within the conception even of Newton himself when he gave utterance to the law: but that the *totality* of a force can be employed according to that law I do not believe, either in relation to gravitation, or electricity, or magnetism, or any other supposed form of power.

I might have drawn reasons for urging a continual recollection of, and reference to, the principle of the conservation of force from other forms of power than that of gravitation; but I think that when founded on gravitating phenomena, they appear in their greatest simplicity; and precisely for this reason, that gravitation has not yet been connected by any degree of convertibility with the other forms of force. If I refer for a few minutes to these other forms, it is only to point in their variations, to the proofs of the value of the principle laid down, the consistency of the known phenomena with it, and the suggestions of research and discovery which arise from it.* *Heat*, for instance, is a mighty form of

* Helmholtz, On the Conservation of Force. Taylor's Scientific Memoirs, 2nd Series, 1853, p. 114.

power, and its effects have been greatly developed; therefore, assumptions regarding its nature become useful and necessary, and philosophers try to define it. The most probable assumption is, that it is a motion of the particles of matter; but a view, at one time very popular, is, that it consists of a particular fluid of heat. Whether it be viewed in one way or the other, the principle of conservation is admitted, I believe, with all its force. When transferred from one portion to another portion of like matter the full amount of heat appears. When transferred to matter of another kind an apparent excess or deficiency often results; the word "capacity" is then introduced, which, whilst it acknowledges the principle of conservation, leaves space for research. When employed in changing the state of bodies, the appearance and disappearance of the heat is provided for consistently by the assumption of enlarged or diminished motion, or else space is left by the term "capacity" for the partial views; which remains to be developed. When converted into mechanical force, in the steam or air-engine, and so brought into direct contact with gravity, being then easily placed in relation to it, still the conservation of force is fully respected and wonderfully sustained. The constant amount of heat developed in the whole of a voltaic current described by M. P. A. Favre,* and the present state of the knowledge of thermo-electricity, are again fine partial or subordinate illustrations of the principle of conservation. Even when rendered radiant, and for the time giving no trace or signs of ordinary heat action, the assumptions regarding its nature have provided for the belief in the conservation of force, by admitting, either that it throws the ether into an equivalent state, in sustaining which for the time the power is engaged; or else, that the motion of the particles of heat is employed altogether in their own transit from place to place.

It is true that heat often becomes evident or insensible in a manner unknown to us; and we have a right to ask what is happening when the heat disappears in one part, as of the thermovoltaic current, and appears in another; or when it enlarges or changes the state of bodies; or what would happen, if the heat, being presented, such changes were purposely opposed. We have a right to ask these questions, but not to ignore or deny the conservation of force; and one of the highest uses of the principle is to suggest such inquiries. Explications of similar points are continually produced, and will be most abundant from the hands of those who, not desiring to ease their labour by forgetting the principle, are ready to admit it either tacitly, or better still, effectively, being then continually guided by it. Such philosophers believe that heat must do its equivalent of work: that if in doing work it seem to disappear, it is still producing its equivalent effect, though often in a manner partially or totally unknown; and that if it give

* Comptes Rendus, 1854, Vol. xxxix., p. 1212.

rise to another form of force (as we imperfectly express it), that force is equivalent in power to the heat which has disappeared.

What is called *chemical attraction*, affords equally instructive and suggestive considerations in relation to the principle of the conservation of force. The indestructibility of individual matter, is one case, and a most important one, of the conservation of chemical force. A molecule has been endowed with powers which give rise in it to various qualities, and these never change, either in their nature or amount. A particle of oxygen is ever a particle of oxygen—nothing can in the least wear it. If it enters into combination and disappears as oxygen,—if it pass through a thousand combinations, animal, vegetable, mineral,—if it lie hid for a thousand years and then be evolved, it is oxygen with its first qualities, neither more nor less. It has all its original force, and only that; the amount of force which it disengaged when hiding itself, has again to be employed in a reverse direction when it is set at liberty; and if, hereafter, we should decompose oxygen, and find it compounded of other particles, we should only increase the strength of the proof of the conservation of force, for we should have a right to say of these particles, long as they have been hidden, all that we could say of the oxygen itself.

Again, the body of facts included in the theory of definite proportions, witnesses to the truth of the conservation of force; and though we know little of the cause of the change of properties of the acting and produced bodies, or how the forces of the former are hid amongst those of the latter, we do not for an instant doubt the conservation, but are moved to look for the manner in which the forces are, for the time, disposed, or if they have taken up another form of force, to search what that form may be.

Even chemical action at a distance, which is in such antithetical contrast with the ordinary exertion of chemical affinity, since it can produce effects miles away from the particles on which they depend, and which are effectual only by forces acting at insensible distances, still proves the same thing, the conservation of force. Preparations can be made for a chemical action in the simple voltaic circuit, but until the circuit be complete that action does not occur; yet in completing we can so arrange the circuit, that a distant chemical action, the perfect equivalent of the dominant chemical action, shall be produced; and this result, whilst it establishes the electrochemical equivalent of power, establishes the principle of the conservation of force also, and at the same time suggests many collateral inquiries which have yet to be made and answered, before all that concerns the conservation in this case can be understood.

This and other instances of chemical action at a distance, carry our inquiring thoughts on from the facts to the physical mode of the exertion of force; for the qualities which seem located and fixed to certain particles of matter appear at a distance in connexion

with particles altogether different. They also lead our thoughts to the *conversion* of one form of power into another: as for instance, in the *heat* which the elements of a voltaic pile may either show at the place where they act by their combustion or combination together; or in the distance, where the electric spark may be rendered manifest; or in the wire or fluids of the different parts of the circuit.

When we occupy ourselves with the dual forms of power, electricity and magnetism, we find great latitude of assumption; and necessarily so, for the powers become more and more complicated in their conditions. But still there is no apparent desire to let loose the force of the principle of conservation, even in those cases where the appearance and disappearance of force may seem most evident and striking. Electricity appears when there is consumption of no other force than that required for friction; we do not know how, but we search to know, not being willing to admit that the electric force can arise out of nothing. The two electricities are developed in equal proportions; and having appeared, we may dispose variously of the influence of one upon successive portions of the other, causing many changes in relation, yet never able to make the sum of the force of one kind in the least degree exceed or come short of the sum of the other. In that necessity of equality, we see another direct proof of the conservation of force, in the midst of a thousand changes that require to be developed in their principles before we can consider this part of science as even moderately known to us.

One assumption with regard to electricity is, that there is an electric fluid rendered evident by excitement in plus and minus proportions. Another assumption is, that there are two fluids of electricity, each particle of each repelling all particles like itself, and attracting all particles of the other kind always, and with a force proportionate to the inverse square of the distance, being so far analogous to the definition of gravity. This hypothesis is antagonistic to the law of the conservation of force, and open to all the objections that have been, or may be, made against the ordinary definition of gravity. Another assumption is, that each particle of the two electricities has a given amount of power, and can only attract contrary particles with the sum of that amount, acting upon each of two with only half the power it could in like circumstances exert upon one. But various as are the assumptions, the conservation of force, (though wanting in the second,) is, I think, intended to be included in all. I might repeat the same observations nearly in regard to magnetism,—whether it be assumed as a fluid, or two fluids or electric currents,—whether the external action be supposed to be action at a distance, or dependent on an external condition and lines of force—still all are intended to admit the conservation of power as a principle to which the phenomena are subject.

The principles of physical knowledge are now so far developed

as to enable us not merely to define or describe the *known*, but to state reasonable expectations regarding the *unknown*; and I think the principle of the conservation of force may greatly aid experimental philosophers in that duty to science, which consists in the enunciation of problems to be solved. It will lead us, in any case where the force remaining unchanged in form is altered in direction only, to look for the new disposition of the force; as in the cases of magnetism, static electricity, and perhaps gravity, and to ascertain that as a whole it remains unchanged in amount:—or, if the original force disappear, either altogether or in part, it will lead us to look for the new condition or form of force which should result, and to develop its equivalency to the force that has disappeared. Likewise, when force is developed, it will cause us to consider the previously existing equivalent to the force so appearing; and many such cases there are in chemical action. When force disappears, as in the electric or magnetic induction after more or less discharge, or that of gravity with an increasing distance; it will suggest a research as to whether the equivalent change is one within the apparently acting bodies, or one *external* (in part) to them. It will also raise up inquiry as to the nature of the internal or external state, both before the change and after. If supposed to be external, it will suggest the necessity of a physical process, by which the power is communicated from body to body; and in the case of external action, will lead to the inquiry whether, in any case, there can be truly action at a distance, or whether the ether, or some other medium, is not necessarily present.

We are not permitted as yet to see the nature of the source of physical power, but we are allowed to see much of the consistency existing amongst the various forms in which it is presented to us. Thus if, in static electricity, we consider an act of induction, we can perceive the consistency of all other like acts of induction with it. If we then take an electric current, and compare it with this inductive effect, we see their relation and consistency. In the same manner we have arrived at a knowledge of the consistency of magnetism with electricity, and also of chemical action and of heat with all the former; and if we see not the consistency between gravitation with any of these forms of force, I am strongly of the mind that it is because of our ignorance only. How imperfect would our idea of an electric current now be, if we were to leave out of sight its origin, its static and dynamic induction, its magnetic influence, its chemical and heating effects? or our idea of any one of these results, if we left any of the others unregarded? That there should be a power of gravitation existing by itself, having *no relation to the other natural powers, and no respect to the law of the conservation of force*, is as little likely as that there should be a principle of levity as well as of gravity. Gravity may be only the residual part of the other forces of nature, as Mossotti has tried to show; but that it should fall out from the law of all other force,

and should be outside the reach either of further experiment or philosophical conclusions, is not probable. So we must strive to learn more of this outstanding power, and endeavour to avoid any definition of it which is incompatible with the principles of force generally, for all the phenomena of nature lead us to believe that the great and governing law is one. I would much rather incline to believe that bodies affecting each other by gravitation act by lines of force of definite amount (somewhat in the manner of magnetic or electric induction, though without polarity), or by an ether pervading all parts of space, than admit that the conservation of force could be dispensed with.

It may be supposed, that one who has little or no mathematical knowledge should hardly assume a right to judge of the generality and force of a principle such as that which forms the subject of these remarks. My apology is this, I do not perceive that a mathematical mind, simply as such, has any advantage over an equally acute mind not mathematical, in perceiving the nature and power of a natural principle of action. It cannot of itself introduce the knowledge of any new principle. Dealing with any and every amount of static electricity, the mathematical mind can, and has balanced and adjusted them with wonderful advantage, and has foretold results which the experimentalist can do no more than verify. But it could not discover dynamic-electricity, nor electro-magnetism, nor magneto-electricity, or even suggest them; though when once discovered by the experimentalist, it can take them up with extreme facility. So in respect of the force of gravitation, it has calculated the results of the power in such a wonderful manner as to trace the known planets through their courses and perturbations, and in so doing has *discovered* a planet before unknown; but there may be results of the gravitating force of other kinds than attraction inversely as the square of the distance, of which it knows nothing, can discover nothing, and can neither assert nor deny their possibility or occurrence. Under these circumstances, a principle, which may be accepted as equally strict with mathematical knowledge, comprehensible without it, applicable by all in their philosophical logic whatever form that may take, and above all, suggestive, encouraging, and instructive to the mind of the experimentalist, should be the more earnestly employed and the more frequently resorted to when we are labouring either to discover new regions of science, or to map out and develop those which are known into one harmonious whole; and if in such strivings, we, whilst applying the principle of conservation, see but imperfectly, still we should endeavour to see, for even an obscure and distorted vision is better than none. Let us, if we can, discover a new thing in *any shape*; the true appearance and character will be easily developed afterwards.

Some are much surprised that I should, as they think, venture to oppose the conclusions of Newton: but here there is a mistake.

I do not oppose Newton on any point ; it is rather those who sustain the idea of action at a distance, that contradict him. Doubtful as I ought to be of myself, I am certainly very glad to feel that my convictions are in accordance with his conclusions. At the same time, those who occupy themselves with such matters ought not to depend altogether upon authority, but should find reason within themselves, after careful thought and consideration, to use and abide by their own judgment. Newton himself, whilst referring to those who were judging his views, speaks of such as are competent to form an opinion in such matters, and makes a strong distinction between them and those who were incompetent for the case.

But after all, the principle of the conservation of force may by some be denied. Well, then, if it be unfounded even in its application to the smallest part of the science of force, the proof must be within our reach, for all physical science is so. In that case, discoveries as large or larger than any yet made, may be anticipated. I do not resist the search for them, for no one can do harm, but only good, who works with an earnest and truthful spirit in such a direction. But let us not admit the destruction or creation of force without clear and constant proof. Just as the chemist owes all the perfection of his science to his dependence on the certainty of gravitation applied by the balance, so may the physical philosopher expect to find the greatest security and the utmost aid in the principle of the conservation of force. All that we have that is good and safe, as the steam-engine, the electric-telegraph, &c., witness to that principle;—it would require a perpetual motion, a fire without heat, heat without a source, action without reaction, cause without effect, or effect without a cause, to displace it from its rank as a law of nature.

[M. F.]

Friday, May 8.

THE LORD WENSLEYDALE, Vice-President, in the Chair.

F. CRACE CALVERT, Esq. F.C.S. M.R.A. Turin,
HONORARY PROFESSOR OF CHEMISTRY, ROYAL INSTITUTION, MANCHESTER.

On M. Chevreul's Laws of Colour.

MR. CRACE CALVERT stated that he had three objects in view in this discourse. The first was to make known the laws of colours, as discovered by his learned master, M. Chevreul; secondly, to explain their importance in a scientific point of view; and, thirdly, their value to arts and manufactures.

To understand the laws of colours, it is necessary to know the composition of light; Newton was the first person who gave to the world any statement relative to the components of light, which he said consisted of seven colours—red, orange, yellow, green, blue, indigo, and violet. It is now distinctly proved that four of those seven colours of the spectrum are the result of the combinations of the three colours now known as the primitive colours, viz., red, blue, and yellow. Thus blue and red combined produce purple or indigo; blue and yellow, green; while red and yellow, produce orange: these facts being known, it is easy to prove that there are not seven, but three primitive, and four secondary, called complementary colours.

Several proofs can be given that light is composed of three colours only. One of the most simple consists in placing pieces of blue, red, and yellow papers on a circular disc, and rotating it rapidly; the effect to the eye being to produce a disc of white light. If, therefore, the eye can be deceived so readily while the disc travels at so slow a rate, what must necessarily be the case when it is remembered that light proceeds at the rate of 190,000 miles per second?

The rapidity with which light travels is such that the eye is not able to perceive either the blue, red, or yellow, the nerves of the retina not being sensitive enough to receive and convey successively to the mind the three or seven colours of which the light is composed.

Before entering into the laws of colour, Mr. Crace Calvert stated that it might be interesting to know what scientific minds had devoted attention to the laws of colours.

Buffon followed Newton, and his researches had special reference to what M. Chevreul had called the "successive contrasts" of colours.

Father Scherffer, a monk, also wrote on the laws of colour. Goethe, the poet, also brought his mind to bear upon the subject, and studied it to a great extent. Count Rumford, about the end of the eighteenth century, published several memoirs on the laws of colours. He explained very satisfactorily the "successive" contrast, and arrived at some insight into the "simultaneous" one; still he did not lay down its real laws.

Prieur, Leblanc, Harris, and Field, were also writers of most interesting works on this subject. The reason that they did not arrive at the definite laws of colour was because they had not divided those laws into successive, simultaneous, and mixed contrasts. These form the basis of the practical laws of colour, and the honour of their discovery is due to M. Chevreul.

The reason why a surface appears white or brilliant is, that a large portion of the light which falls on its surface is reflected on the retina, and in such a quantity as gives to the surface a brilliant aspect; whilst in plain white surfaces, the rays of light being diffused in all directions, and a small portion only arriving to the eye, the surface does not appear brilliant. The influence of colours on these two kinds of surfaces is very different, as may be perceived by the examples round the room, showing the influence of different colours on gold ornaments. When rays of light, instead of being reflected, are absorbed by a surface or substance it appears black; therefore white and black are not colours, as they are due to the reflection or absorption of undecomposed light. It is easy to understand why a surface appears blue; it is due to the property which the surface has to reflect only blue rays, whilst it absorbs the yellow and red rays; and if a certain portion of light is reflected with one of the coloured rays it will decrease its intensity; thus red rays with white ones produce pink. On the contrary, if a quantity of undecomposed light is absorbed, black is produced, which, by tarnishing the colour and making it appear darker, generates dark reds, blues, or yellows. The secondary colours are produced by one of the primitive colours being absorbed and the two others reflected; for example, if red be absorbed, and blue and yellow reflected, the surface appears green. There are two reasons why a perfect blue, yellow, red, cannot be seen, &c. The first is, that surfaces cannot entirely absorb one or two rays and reflect the others. The second is, that when the retina receives the impression of one colour, immediately its complementary colour is generated; thus, if a blue circle is placed on a perfectly grey surface, an orange hue will be perceived round it; if an orange circle, round it will be noticed a bluish tint; if a red circle, a green; if a greenish yellow circle, a violet; if an orange yellow circle, an indigo; and so on.

The "successive" contrast has long been known ; and it consists in the fact that on looking stedfastly for a few minutes on a red surface fixed on a white sheet of paper, and then carrying the eye to another white sheet, there will be perceived on it not a red, but a *green* one ; if *green*, *red* ; if *purple*, *yellow* ; if *blue*, *orange*.

The "simultaneous" contrast is the most interesting and useful to be acquainted with. When two coloured surfaces are in juxtaposition, they mutually influence each other,—favourably, if harmonising colours, or in a contrary manner if discordant ; and in such proportion in either case as to be in exact ratio with the quantity of complementary colour which is generated in the eye : for example, if two half-sheets of plain tinted paper, one dark *green*, the other of a brilliant *red*, are placed side by side on a grey piece of cloth, the colours will be mutually improved in consequence of the *green* generated by the *red* surface adding itself to the *green* of the juxtaposed surface, thus increasing its intensity, the *green* in its turn augmenting the beauty of the *red*. This effect can easily be appreciated if two other pieces of paper of the same colours are placed at a short distance from the corresponding influenced ones, as below :—

Red. Red Green. Green.

It is not sufficient merely to place complementary colours side by side to produce harmony of colour, since the respective intensities have a most decided influence : thus pink and light green agree, red and dark green also ; but light green and dark red, pink and dark green, do not ; and thus to obtain the maximum of effect and perfect harmony the following colours must be placed side by side, taking into account their exact intensity of shade and tint.

HARMONISING COLOURS.

<i>Primitive Colours.</i>	<i>Complementary Colours.</i>	
Red	Green	<div style="display: inline-block; vertical-align: middle;"> <div style="display: inline-block; vertical-align: middle;"> <div>Light blue</div> <div>Yellow</div> <div>Red</div> </div> <div style="font-size: 3em; vertical-align: middle; margin: 0 5px;">}</div> <div>White light.</div> </div>
Blue	Orange	<div style="display: inline-block; vertical-align: middle;"> <div style="display: inline-block; vertical-align: middle;"> <div>Red</div> <div>Yellow</div> <div>Blue</div> </div> <div style="font-size: 3em; vertical-align: middle; margin: 0 5px;">}</div> <div>White light.</div> </div>
Yellow-orange	Indigo	<div style="display: inline-block; vertical-align: middle;"> <div style="display: inline-block; vertical-align: middle;"> <div>Blue</div> <div>Red</div> <div>Yellow</div> </div> <div style="font-size: 3em; vertical-align: middle; margin: 0 5px;">}</div> <div>White light.</div> </div>
Greenish Yellow	Violet	<div style="display: inline-block; vertical-align: middle;"> <div style="display: inline-block; vertical-align: middle;"> <div>Red</div> <div>Blue</div> <div>Yellow</div> </div> <div style="font-size: 3em; vertical-align: middle; margin: 0 5px;">}</div> <div>White light.</div> </div>
Black	White	<div style="display: inline-block; vertical-align: middle;"> <div style="display: inline-block; vertical-align: middle;"> <div>Yellow</div> <div>Blue</div> <div>Red</div> </div> <div style="font-size: 3em; vertical-align: middle; margin: 0 5px;">}</div> <div>White light.</div> </div>

If attention is not paid to the arrangement of colours according to the above diagram, instead of their mutually improving each other, they will, on the contrary, lose in beauty ; thus if blue and purple are placed side by side, the blue throwing its complementary colour, orange, upon the purple, will give it a faded appearance ; and the blue receiving the orange yellow of the purple will assume a greenish tinge. The same may be said of yellow and red, if placed in juxtaposition. The red, by throwing its complementary colour green, on the yellow, communicates to it a greenish tinge ; the yellow, by throwing its purple hue, imparts to the red a disagreeable purple appearance. The very great importance of these principles to every one who intends to display or arrange coloured goods or fabrics was convincingly shown by Mr. Crace Calvert, from a great variety of embroidered silks (kindly lent by Mr. Henry Houldsworth), calicos, and paper-hangings, which demonstrated that if these laws are neglected, not only will the labour and talent expended by the manufacturer to produce on a given piece of goods the greatest effect possible, be neutralised, but perhaps lost. It was clearly demonstrated that these effects are not only produced by highly-coloured surfaces, but also by those whose colours are exceedingly pale, as, for example, light greens, or light blues with buffs, and that even in gray surfaces, as pencil drawings, the contrast of tone between two shades was distinctly visible. The contrast of tone or tint was most marked when two tints of the same colour were juxtaposed, and it was therefore the interest of an artist to pay attention to this principle when employing two tints of the same scale of colour. From the "mixed contrast" arises the rule that a brilliant colour should never be looked at for any length of time, if its true tint or brilliancy is to be appreciated ; for if a piece of red cloth is looked at for a few minutes, green, its complementary colour, is generated in the eye, and adding itself to a portion of the red, produces black, which tarnishes the beauty of the red. This contrast explains, too, why the tone of a colour is modified, either favourably or otherwise, according to the colour which the eye has previously looked at. Favourably, when, for instance, the eye first looks to a yellow surface, and then to a purple one ; and unfavourably, when it looks at a blue and then at a purple.

Mr. Crace Calvert also showed that black and white surfaces assume different hues according to the colours placed in juxtaposition with them ; for example, black acquires an orange or purple tint if the colours placed beside it are blue or orange ; but these effects can be overcome, in the case of these or any colours, by giving to the influenced colour a tint similar to that influencing it. Thus, to prevent black becoming orange by its contact with blue, it is merely necessary that the black should be blued, and in such proportion that the amount of blue will neutralize the orange thrown on it by influence, thus producing black. As an instance,

to prevent a grey design acquiring a pinkish shade through working it with green, give the grey a greenish hue, which, by neutralising the pink, will generate white light, and thus preserve the grey.

Mr. Crace Calvert, after explaining the chromatic table of M. Chevreul, which enabled any person at a glance to ascertain what was the complementary colour of any of the 13,480 colours which M. Chevreul had distinctly classed in his table, stated that it was of the highest importance to artists to be acquainted with these laws, in order to know at once the exact colour, shade, and tint, which would produce the greatest effect when placed beside another colour, and that they could save the great length of time which no doubt the great masters lost in ascertaining by experiment those laws, which they could now learn in a few hours by consulting M. Chevreul's work.

[F. C. C.]

Friday, June 5.

THE DUKE OF NORTHUMBERLAND, K.G. F.R.S. President,
in the Chair.

JOHN TYNDALL, Esq F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY, B.I.

On M. Lissajous' Acoustic Experiments.

THE speaker briefly noticed the physical cause of musical sound ; referring to the bell, the tuning fork, the tended string, &c., as sources of vibration. The propagation of impulses through the atmosphere to the tympanum was illustrated by causing a brass rod to vibrate longitudinally : a disk was fixed to the end of the rod perpendicular to its length, and this disk, being held several feet above a surface of stretched paper on which sand was strewn, communicated its motion through the air to the paper, and produced a complex nodal figure of great beauty. Optical means had been resorted to by Dr. Young, and more especially by Mr. Wheatstone, in the study of vibratory movements. M. Lissajous had extended and systematised the principle ; and had exhibited his experiments before the Société d'Encouragement, and more recently before the Emperor of the French. When he became acquainted with the speaker's intention to introduce these experiments at the Royal Institution, he in the most obliging manner offered to come to London and make them himself. This offer was accepted, and the speaker also congratulated the audience on the presence of M. Duboscq, who took charge of his own electric lamp ; this being the source of light made use of on the occasion.

The experiments proceeded in the following order :—

1. A sheaf of light was thrown from the lamp upon a mirror held in the speaker's hand : on moving the mirror with sufficient speed the beam described a luminous ring upon the ceiling. The persistence of impressions upon the retina was thus illustrated.

2. A tuning fork had a pointed bit of copper foil attached to one of its prongs : the fork being caused to vibrate by a violin bow the metallic point moved to and fro, and being caused to press gently upon a surface of glass coated with lamp black, the fork being held still, a fine line of a length equal to the amplitude of the vibrations was described upon the glass ; but when at the same time the whole fork was drawn backwards with sufficient speed, a sinuous line was described upon the glass. The experiment was made by placing the coated glass before the lamp ; having a lens

in front of it, and bringing the surface of the glass to a focus on a distant screen. On drawing the fork over the surface in the manner described, the figure started forth with great beauty and precision. By causing a number of forks to pass at the same time over the coated glass, the relations of their vibrations were determined by merely counting the sinuosities. The octave, for example, had double the number of its fundamental note.

3. This was the first of the series of M. Lissajous' experiments. A tuning fork, with a metallic mirror attached to one of its prongs, was placed in front of the lamp; an intense beam of light was thrown on the mirror, and reflected back by the latter. This reflected beam was received on a small looking-glass, held in the hand of the experimenter, from which it was reflected back upon the screen. A lens being placed between the lamp and tuning fork, a sharply defined image of the orifice from which the light issued was obtained. When a violin bow was drawn across the fork, this image elongated itself to a line. By turning the mirror in the hand, the image upon the screen was resolved into a bright sinuous track, many feet in length.

4. A tuning fork was placed before the lamp, as in the last experiment. But instead of receiving the beam reflected from the mirror of the fork upon a looking-glass, it was received upon the mirror of a second fork, and reflected by the latter upon the screen. When one fork was excited by a bow, a straight line described itself upon the screen, when the other fork was subsequently excited, the figure described was that due to the combination of the vibrations of both the forks. This is the principle of the entire series of experiments now to be referred to.

When a single fork vibrates, the image which it casts upon the screen is elongated in a direction parallel to the prong of the fork. In order to have the vibrations rectangular, one fork stood upright, the other was fixed horizontally, in a vertical stand, in the following experiments.

5. Two forks, in perfect unison with each other, were placed in the positions described, and caused to vibrate simultaneously. If both forks passed their position of equilibrium at the same instant, that is, if there was no difference of phase, the figure described was a straight line. When the difference of phase amounted to one-fourth, the figure was a circle: between these it was an ellipse. The perfect unison of the two forks was proved by the immobility of the figure upon the screen. On loading one of them with a little weight, the figure no longer remained fixed but passed from the straight line through the ellipse to a circle, thence back through the ellipse to the straight line. So slight is the departure from unison which may be thus rendered visible, that M. Lissajous states that it would be possible to make evident to a deaf person a discrepancy of one vibration in thirty thousand.

6. Two forks, one of which gave the octave of the other, were

next made use of. When there was no difference of phase, the figure described upon the screen resembled an 8. If the unison was perfect, the figure, as in the former case, was fixed ; but when the unison was disturbed, the figure passed through the changes corresponding to all possible differences of phase. The loops of the 8 became distorted, formed by superposition a single parabola, opened out again, became again symmetrical, and so on.

7. The fifth of the octave, the major third, and other combinations succeeded, the figures becoming more and more complex as the departure from simple relations between the vibrations increased.

8. Finally, two forks which, when sounded together, gave audible beats, were placed both upright upon the table. The beam reflected from the mirror of one was received upon that of the other, and reflected upon the screen. When both forks were sounded, they sometimes conspired to elongate the image ; sometimes they opposed each other, and thus a series of elongations and shortenings addressed the eye at exactly the same intervals in which the beats addressed the ear.

At the conclusion of this beautiful series of experiments, which, thanks to the skill of those who performed them, were all successful, on the motion of Mr. Faraday, the thanks of the meeting were unanimously voted to M.M. Lissajous and Dubosecq, and communicated to those gentlemen by his Grace the President.

[J. T.]

Friday, June 12.

SIR BENJAMIN COLLINS BRODIE, BART. D.C.L. F.R.S. Vice-President, in the Chair.

PROFESSOR FARADAY, D.C.L. F.R.S.

On the Relations of Gold to Light.

THIS subject was brought forward on the 13th of June of last year, and in the account of that evening, at page 310, vol. ii. of the Proceedings of the Royal Institution, will be found a description of some of the proofs and effects then referred to and illustrated; the following additional remarks will complete the account up to this time. The general relations of *gold leaf* to light were described in the former report. Since then, pure gold leaf has been obtained through the kindness of Mr. Smirke, and the former observations verified. This was the more important in regard to the effect of heat in taking away the green colour of the transmitted light, and destroying to a large extent the power of reflexion. The temperature of boiling oil, if continued long enough, is sufficient for this effect; but a higher temperature (far short of fusion) produces it more rapidly. Whether it is the result of a mere breaking up by retraction of a corrugated film, or an allotropic change, is uncertain. Pressure restores the green colour; but it also has the like effect upon films obtained by other processes than beating. Corresponding results are produced with other metals.

As before stated, *films* of gold may be obtained on a weak solution of the metal, by bringing an atmosphere containing vapours of phosphorus into contact with it. They are produced also when small particles of phosphorus are placed floating on such a solution; and then, as a film differing in thickness is formed, the concentric rings due to Newton's thin plates are produced. These films transmit light of various colours. When heated they become amethystine or ruby; and then when pressed, become green, just as heated gold leaf. This effect of pressure is characteristic of metallic gold, whether it is in leaf, or film, or dust.

Gold wire, separated into very fine particles by the electric *deflagration*, produces a deposit on glass, which, being examined, either chemically or physically, proves to be pure metallic gold.

This deposit transmits various coloured rays : some parts are grey, others green or amethystine, or even a bright ruby. In order to remove any possibility of a compound of gold, as an oxide, being present, the deflagrations were made upon topaz, mica, and rock crystal, as well as glass, and also in atmospheres of carbonic acid and of hydrogen. Still the results were the same, and ruby gold appeared in one case as much as in another. Being heated, all parts of the deposit became of an amethystine or ruby colour ; and by pressure these parts could be changed so as to transmit the green ray.

The production of *fluids*, consisting of very finely divided particles of gold diffused through water, was spoken of before. These fluids may be of various colours by transmitted light from ruby to blue ; the effects being produced only by diffused particles of metallic gold. If a drop of solution of phosphorus in bisulphide of carbon be put into a bottle containing a quart or more of very weak solution of gold, and the whole be agitated, the change is brought about sooner than by the process formerly described ; or if a solution of phosphorus in ether be employed, very quickly indeed ; so that a few hours' standing completes the action. All the preparations have the same qualities as those before described. The differently coloured fluids may have the coloured particles partially removed by filtration ; and so long as the particles are kept by the filter from aggregation, they preserve their ruby or other colour unchanged, even though salt be present. If fine isinglass be soaked in water, then warmed to melt it, and one of these rich fluids be added, with agitation, a ruby jelly fluid will be obtained, which, when sufficiently concentrated and cold, supplies a tremulous jelly ; and this, when dried, yields a *hard ruby gelatine*, which being soaked in water, becomes tremulous again, and by heat and more water yields a ruby fluid. The dry hard ruby jelly is perfectly analogous to the well known ruby glass, though often finer in colour ; and both owe the colour to particles of metallic gold. Animal membranes may in like manner have ruby particles diffused through them, and then are perfectly analogous in their action on light to the gold ruby glass, and from the same cause.

When a leaf of beaten gold is held obliquely across a ray of common light, it *polarizes* a portion of it ; and the light transmitted is polarized in the same direction as that transmitted by a bundle of thin plates of glass ; the effect is produced by the heated leaf as well as by the green leaf, and does not appear to be due to any condition brought on by the heating or to internal structure. When a polarized ray is employed, and the inclined leaf held across it, the ray is affected, and a part passes the analyzer, provided the gold film is inclined in a plane forming an angle of 45° with the plane of polarization. Like effects are produced by the films of gold produced from solution and phosphorus, and also by the deposited dust of gold due to the electric discharge. The same effects are

produced by the other deflagrated metals so long as the dusty films are in the metallic state. As these finer preparations could be held in place only on glass or some such substance, and as glass itself had an effect, it was necessary to find a medium in which the power of the glass was nothing; and this was obtained in the bisulphide of carbon. Here the effect of gold upon a ray of light which was unaffected by the glass supporting it, was rendered very manifest, not only to a single observer, but also to a large audience.

The object of these investigations was to ascertain the varied powers of a substance acting upon light, when its particles were extremely divided, to the exclusion of every other change of constitution. It was hoped that some of the very important differences in the action upon the rays might in this way be referred to the relation in size or in number of the vibrations of the light and the particles of the body, and also to the distance of the latter from each other: and as many of the effects are novel in this point of view, it is hoped that they will be of service to the physical philosopher.

[M. F.]

Friday, January 22.

WILLIAM POLE, Esq. M.A. F.R.S. Treasurer and Vice-President,
in the Chair.

PROFESSOR J. TYNDALL, F.R.S.

On some Physical Properties of Ice.

THE discourse was prefaced by some remarks on force in general; and more especial attention was afterwards directed to the force, or application of force, manifested in the phenomena of crystallization. Experimental illustrations were exhibited, and the speaker passed on to the particular case of crystallized water, or ice. Being desirous of examining how the interior of a mass of ice was affected by a beam of radiant heat sent through it, he availed himself of the sunny weather of last September and October. The sunbeams being condensed by a lens, the concentrated beams were sent through slabs of ice, the arrangement being usually so made as to cause the focus to fall within the substance. The path of such a beam through the ice was observed to be instantly studded with lustrous spots, which increased in magnitude and number as the action continued. On examining the spots more closely, they were found to be flattened spheroids, and around each the ice was so liquified as to form a beautiful flower-shaped figure, possessing six petals. From this number there was no deviation. At first the edges of the liquid leaves were clearly defined; but a continuance of the action usually caused the edges to become serrated like those of ferns. When the ice was caused to move across the beam, or the beam caused to traverse different portions of the ice in succession, the sudden generation and crowding together of these liquid flowers, with their central spots shining with more than metallic brilliancy, was exceedingly beautiful.

A slab of ice was prepared, and placed in front of an electric lamp: by a lens placed in front of the slab, the latter was projected upon a screen; on sending a beam from the lamp through the ice, the formation of the flowers was rendered visible to the audience.

In almost all cases these flowers were formed in planes parallel to the surface of freezing; it mattered not whether the beam traversed the ice parallel to this surface, or perpendicular to it. Some apparent exceptions to this rule were found, which will form the subject of future investigation.

The general appearance of the shining spots at the centres of the flowers was that of the bubbles of air entrapped in the ice: to examine whether they contained air or not, portions of ice containing them were immersed in warm water. The ice surrounding the cavities melted, the latter instantly collapsed, and no trace of air rose to the surface of the water. A vacuum, therefore, had been formed at the centre of each spot; due, doubtless, to the well-known fact that the volume of water in each flower was less than that of the ice, by the melting of which the flower was produced.

The associated air-and-water cells, found in such numbers in the ice of glaciers, and observed by the speaker in lake ice, were next examined. Two hypotheses have been started to account for these cells. One attributes them to the absorption of the sun's heat by the air bubbles, and the consequent melting of the ice which surrounds them. The other hypothesis, which is a very reasonable one, supposes that the liquid in the cells never has been frozen, but has continued in the liquid condition from the *névé* or origin of the glacier downwards. Now if the water in the cells be due to the melting of the ice, the associated air must be in a *rarefied condition*, because the volume of the liquid is less than that of the ice which produced it; whereas, if the air be simply that entrapped in the snow of the *névé*, it will not be thus rarefied. Here, then, we have a test as to whether the water-cells have been produced by the melting of the ice.

Portions of ice containing these compound cells were immersed in hot water, the ice around the cavities being thus gradually melted away. When a liquid connexion was established between the bubble and the atmosphere, the former collapsed to a smaller bubble. In many cases the residual bubble did not reach the hundredth part of the magnitude of the primitive one. There was no exception to this rule, and it proves that the water of the cavities is really due to the melting of the adjacent ice.

The first hypothesis above referred to is that of M. Agassiz; which has been reproduced and subscribed to by the Messrs. Schlagintweit, and accepted generally as the true one. Let us pursue it to its consequences.

Comparing equal *weights* of air and water, experiment proves that to raise a given weight of water one degree in temperature, as much heat would be needed as would raise the same weight of air four degrees.

Comparing equal *volumes* of air and water, the water is known to be 770 times heavier than the air; consequently, for a given *volume* of air to raise an equal *volume* of water one degree in temperature, it must part with $770 \times 4 = 3080$ degrees.

Now the quantity of heat necessary to melt a given weight of ice would raise the same weight of water 142.6 Fahr. degrees in temperature. Hence to produce, by the melting of ice, an amount of water equal to itself in bulk, a bubble of air must yield up

3080×142.6 , or upwards of four hundred thousand degrees Fahrenheit.

This is the amount of heat which, according to the hypothesis of M. Agassiz and the Messrs. Schlagintweit, is absorbed by the bubble of the air in a short time under the eyes of the observer. That is to say, the air is capable of absorbing an amount of heat which, had it not been communicated to the surrounding ice, would raise the bubble to a temperature 160 times that of fused cast iron. Did air possess this enormous power of absorption it would not be without inconvenience for the animal and vegetable life of our planet.

The fact is, that a bubble of air at the earth's surface is unable, in the slightest appreciable degree, to absorb the sun's rays; for those rays before they reach the earth have been perfectly sifted by their passage through the atmosphere. The following experiment illustrative of this point, has been made by the speaker: the rays from an electric lamp were condensed by a lens, and the concentrated beam sent through the bulb of a differential thermometer. The heat of the beam was intense; still not the slightest effect was produced upon the thermometer. In fact, all the rays that glass could absorb had been *absorbed by the lens*, and the heat consequently passed through the thin glass envelope of the thermometer, and the air within it, without imparting the slightest sensible heat to either.

The bubbles observed by the speaker, and those which occur in the deeper portions of glacier ice, he supposes to have been produced by heat which has been *conducted* through the substance without melting it. Regarding heat as a mode of motion, he shows that the liberty of liquidity is attained by the molecules at the surface of a mass of ice, before the molecules at the centre of the mass can attain this liquidity. Within the mass each molecule is controlled in its motion by the surrounding molecules. But if a cavity exist at the interior, the molecules surrounding that cavity are in a condition similar to those at the surface; and they are liberated by an amount of motion which has been transmitted through the ice without prejudice to its solidity. The conception is helped when we call to mind the transmission of motion through a series of elastic balls, by which the last ball of the series is detached, while the others do not suffer visible separation. The speaker, moreover, proves, by actual experiment, that the interior portion of a mass of ice may be liquified by an amount of heat which has been conducted through the exterior portions without melting them.

Now precisely the converse of this takes place when two pieces of ice, at 32° Fahr., with moist surfaces, are brought into contact. Superficial portions are by this act transferred to the centre, where a temperature of 32° is not sufficient to produce liquefaction. The motion of liquidity which the surfaces possessed before contact is

now checked, and the pieces of ice freeze together. This appears to furnish a complete explanation of all the cases of this nature which have hitherto been observed.

The particles of a crushed mass of ice at 32° , or a ball of moist snow, may, it is now well known, be squeezed into slabs or cups of ice. That moisture is necessary here, and that the same agent is necessary in the conversion of snow into glacier ice, was proved by the following experiment. A ball of ice was cooled in a bath of solid carbonic acid and ether, and thus rendered perfectly dry. Placed in a suitable mould, and subjected to hydraulic pressure, the ball was crushed; but the crushed fragments remained as *white and opaque* as those of crushed glass. The particles, while thus dry, could not be squeezed so as to form pellucid ice, which is so easily obtained when the compressed mass is at a temperature of 32° Fahr.

[J. T.]

Friday, January 29.

SIR BENJAMIN COLLINS BRODIE, Bart., D.C.L. F.R.S.
Vice-President, in the Chair.

WILLIAM ROBERT GROVE, Esq. Q.C. V.P.R.S.
On Molecular Impressions by Light and Electricity.

THE term *molecule* is used in different senses by different authors: by some it is employed with the same meaning as the word *atom*, i.e., to signify an ultimate indivisible particle of matter; by others to signify a definite congeries of atoms forming an integral element of matter, somewhat as a brick may be said to be a congeries of particles of sand, but a structural element of a house.

The term is used this evening to signify the particles of bodies smaller than those having a sensible magnitude, or only as a term of contradistinction from masses. If there be any distinctive characteristic of the science of the present century as contrasted with that of former times, it is the progress made in molecular physics, or the successive discoveries which have shown that when ordinary ponderable matter is subjected to the action of what were formerly called the imponderables, the matter is molecularly changed. The remarkable relations existing between the physical structure of matter, and its effect upon heat, light, electricity, magnetism, &c., seems, until the present century, to have attracted little attention: thus, to take the two agents selected for this evening's discourse, Light and Electricity, how manifestly their effects depend upon the molecular structure of the bodies subjected to their influence? Carbon in the form of diamond transmits light but stops electricity. Carbon in the form of coke or graphite, into which the diamond may be transformed by heat, transmits electricity but stops light. All solid bodies which transmit light freely, or are transparent, are non-conductors of electricity, or may be said to be opaque to it; all the best conductors of electricity, as black carbon and the metals, are opaque or non-conductors of light.* Bodies which have a peculiar but definite and symmetrical structure, such as crystals, affect light definitely and in strict relation to their structure: witness the effects of polarized light on crystals; and there are not wanting instances

* It should be borne in mind that these terms are not absolute, but only express a high degree of approximation.

of similar relations between the structure of bodies and their transmission of electricity.

The converse of this class of effects, however, forms more properly the subject of this evening's communication, viz., the changes in the molecular structure of matter produced by Light and Electricity. The effect of light on plants, on their growth and colour, the bleaching effects of light on coloured bodies, the phosphorescence of certain substances by insolation or exposure to the sun, have long been known, and yet do not seem to have awakened in the minds of the ancient natural philosophers any notion of the general molecular effects of light. Leonard Euler alone conceived that light may be regarded as a movement or undulation of ordinary matter; and Dr. Young, in answer, stated as a most formidable objection, that if this view were correct all bodies should possess the properties of solar phosphorus, or should be thrown into a state of molecular vibration by the impact of light, just as a resonant body is thrown into vibration by the impact of sound, and thus give back to the sentient organ an effect similar to that of the original impulse.

In the last edition of his Essay on the "Correlation of Physical Forces," (1855, p. 131,) Mr. Grove has made the following remarks on this question: "To the main objection of Dr. Young that all bodies would have the properties of solar phosphorus if light consisted in the undulations of ordinary matter, it may be answered that so many bodies have this property, and with so great variety in its duration, that *non constat* all may not have it, though for a time so short that the eye cannot detect its duration; the fact of the phosphorescence by insolation of a large number of bodies is in itself evidence of the matter of which they are composed being thrown into a state of undulation, or at all events molecularly affected by the impact of light, and is therefore an argument in support of the view to which objection is taken." The above conjecture has been substantially verified by the recent experiments of M. Niepce de St. Victor, of which the following is a short *resumé*:—

An engraving which has been for some time in the dark is exposed to sunlight as to one half, the other half being covered by an opaque screen: it is then taken into a dark room, the screen removed, and the whole surface placed in close proximity to a sheet of highly sensitive photographic paper. The portion upon which the light has impinged is reproduced on the photographic paper, while no effect is produced by the portion which had been screened from light. White bodies produce the greatest effect, black little or none, and colours intermediate effects.

An engraving exposed as before, then placed in the dark upon white paper, conveys the impression to the latter, which will in its turn impress photographic paper.

Paper, in a tin case, exposed to sunlight, then covered up by a tin cover will, when opened in the dark, radiate from the aperture

phosphorescent force, and produce a circular mark on the photographic paper, and even impress on the latter the lines of an engraving interposed between it and the photographic surface.

Phosphorescent bodies produce similar effects in a greater degree, and bodies which intercept the phosphorescent effect intercept the invisible radiations. A design drawn by a fluorescent substance, such as a solution of sulphate of quinine on paper, is reproduced, the design being more strongly impressed than the residual parts of the paper.

Mr. Grove had little doubt that had the discourse been given in the summer instead of mid-winter, he could have literally realised in this theatre the Lagado problem of extracting sunbeams from cucumbers!

While fishing in the autumn, in the grounds of M. Seguin, at Fontenay, Mr. Grove observed some white patches on the skin of a trout, which he was satisfied had not been there when the fish was taken out of the water. The fish having been rolling about in some leaves at the foot of a tree, gave him the notion that the effect might be photographic, arising from the sunlight having darkened the uncovered, but not the covered portions of the skin. With a fresh fish a serrated leaf was placed on each side, and the fish laid down so that the one side should be exposed, the other sheltered from light: after an hour or so the fish was examined, and a well defined image of the leaf was apparent on the upper or exposed side, but none on the under or sheltered side. There was no opportunity of further experiment; but there seems little doubt of the effect being photographic, or an oxidation or deoxidation of the tissue determined by light.

Many important considerations might be suggested as deducible from the above results, as to the influence of light on health, both that of vegetables and animals. The effect of light on the healthy growth of plants is well known; and it is generally believed that dark rooms, though well heated and ventilated, are more "close" or less healthy than those exposed to light. When we consider the invisible phosphorescence which must radiate from the walls and furniture, when we consider the effects of light on animal tissue, and the probable ozonizing or other minute chemical changes in the atmosphere effected by light, it becomes probable that it is far more immediately influential on the health of the animate world than is generally believed.

The number of substances proved to be molecularly affected by light is so rapidly increasing, that it is by no means unreasonable to suppose that all bodies are in a greater or less degree changed by its impact.

Passing now to the effects of Electricity, every day brings us fresh evidence of the molecular changes effected by this agent. The electric discharge alters the constitution of many gases across which it is passed; and it was shown, that by passing it through

an attenuated atmosphere of the vapours of phosphorus, this element is changed by the electric discharge into its allotropic variety, which is deposited in notable quantity on the sides of the receiver. In this experiment, the transverse bands or striæ discovered by Mr. Grove, in 1852, are very strikingly shown. Not only is the gaseous intermedium thus affected, but the terminals from which the discharge appears to issue, are disintegrated, and their molecules projected. Some tubes, through the interior of which Mr. Gassiot had passed the discharge from Ruhmkorff's coil for a considerable time, were shown to be coated in the interior, for a notable space around the negative terminal, with a deposit of platinum, forming a reflecting surface like the back of a looking-glass. The vacuum in these tubes was Torricellian, the tubes having been hermetically sealed after the descent of the mercury, so as to cut them off from the mercurial surface. In these cases the electric discharge passes from metal to metal; but the glow which is seen on excited electrics, such as glass, was also shown by Mr. Grove to be accompanied with molecular change. Letters cut in paper, and placed between two well cleaned sheets of glass, formed into a Leyden apparatus by sheets of tin foil on their outer surfaces, and then electrified by connexion for a few seconds with a Ruhmkorff coil, had invisible images of the letters impressed upon the interior surfaces, which were rendered visible by breathing on them; and rendered visible, and at the same time permanently etched, by exposure, after electrization, to the vapour of hydrofluoric acid.

So, again, if iodized collodion be poured over the surface of glass having the invisible image, and then treated as for a photograph, and exposed to uniform daylight, the invisible image is ultimately developed in the collodion film; the invisible molecular change having been conveyed to the collodion, and rendering it, when nitrated, more sensitive to light in the parts where it has been in proximity to the electrical impression, than in the residual parts. Here we have a molecular change, produced first by electricity on the glass, then communicated by the glass to the collodion, then changed in character by light, and all this time invisible; and then rendered visible by pyrogallie acid, the developing chemical agent. Test papers between the plates of glass so electrized, show an acid, and also a bleaching re-action, probably due to the formation of nitrous acid and of ozone; and thus evidencing a chemical change in the elastic intermedium, as well as in the bounding surfaces: but the interior molecules of the glass appear also to partake of the effect, as the impressions are reproduced in many cases on the opposite surface of the glass.

Mr. Babbage had observed that some plates of glass which had formed the ornamented margin of an old looking-glass, and were backed by a design in gold leaf covered with plaster of Paris, showed, when this backing was removed by soft soap, an impression of the gold-leaf device, which was rendered visible by the breath on

the glass. Some of the plates had been kindly lent by him for this evening; and in one, Mr. Grove had removed a portion of the backing, and the continuation of the gilded design came beautifully out by breathing on the glass while in the frame of the electric lamp, and was projected (as were the previous electrical images) on a white screen. The effect on Mr. Babbage's plates may be also electrical, arising from the gold—a good conductor—acting as platinum does in the voltaic battery, and setting up a chemical action between the substance used for making the gold adhere and the glass, or between the constituents of the glass itself; but it would be hazardous, without further experiment, to express any confident opinion on this point.

Of the practical results to science of the molecular changes forming the subject of this evening's discourse, a beautiful illustration was afforded by the photographs of the moon by Mr. Warren De la Rue, which gave, by the aid of the electric lamp, images of the moon of six feet diameter, in which the details of the moon's surface were well defined,—the cone in Tycho, the double cone in Copernicus, and even the ridge of Aristarchus, could be detected. The bright lines, radiating from the mountains, were clear and distinct. A photograph of the planet Jupiter was also shown, in which the belts were very well marked, and the satellites visible. The following question was suggested by Mr. Grove. As telescopic power is known to be limited by the area of the speculum or object glass, even assuming perfect definition, as the light decreases inversely as the square of the magnifying power, a limit must be reached at which the minute details of an object become lost for want of light. Now, assuming a high degree of perfection in astronomical photographs, these may be illuminated to an indefinite degree of brilliancy by adventitious light. With a given telescope, could a better effect be obtained by illuminating the photographic image, and applying microscopic power to that, than by magnifying the luminous image in the usual way by the eye-glass of the telescope? Can the addition of extraneous light to the photograph permit a higher magnifying power to be used with effect than that which can be used to look at the image which makes the photographic impression? In other words, is the photographic eye more sensitive than the living eye; or can a photographic recipient be found which will register impressions which the living eye does not detect, but which, by increased light or by developing agents, may be rendered visible to the living eye? Much may be said, *pro* and *con*, on this question, and it probably can only be satisfactorily answered by experiment, when photographic science is sufficiently advanced.

The phenomena treated of this evening, which are a mere selection from a crowd of analogous effects, show that light and electricity, in numerous cases, produce a molecular change in ponderable matter affected by them. The modifications of the supposed imponderables themselves have long been the subjects of investigation;

the recent progress of science teaches us to look for the reciprocal effects on the matter affected by them.

Gases which have transmitted *light* are altered; as, for example, chlorine is rendered capable of combining directly with hydrogen; liquids are altered, peroxalate of iron is chemically changed, and gives off carbonic acid; and the light which has produced these effects is less able to produce them a second time. Solids are altered, as shown in the extensive range of photographic effects. So with *electricity*,—compound gases are changed chemically, as ammonia or atmospheric air; elementary gases are changed allotropically, as phosphorus vapour, or oxygen; liquids are changed, as in the decomposition of water and other electrolytes; and solids are changed, as in the projection of the particles of the terminals, and the impressions on the surfaces of electrics, shown this evening. Frictional electricity may itself be due to the rupture of cohesion between dissimilar molecules; at all events few, if any, electrical effects have not been proved to be accompanied with molecular changes; and we are daily receiving additions to those produced by light. So, again, iron, and other bodies, have their molecular structure changed by magnetism. Chemical affinity is universally, and heat generally, admitted to be an affection of ordinary matter. Mr. Grove feels deeply convinced that a dynamic theory, one which regards the imponderables as forces acting upon ordinary matter in different states of density, or as modes of motion, and not as fluids or entities, is the truest conception which the mind can form of these agents; but to those who are not willing to go so far, the ever increasing number of instances of such molecular changes affords a boundless field of promise for future investigation, for new physical discoveries and new practical applications.

The permanency of such changes also gives valuable means of reading, in the present state of matter, its past history: final or absolute knowledge on such subjects we cannot hope to obtain, but relative or approximate knowledge is as unlimited as is the degree of improvement in the powers attainable for its acquisition.

Note.—Since the above was written, the author has observed a case of molecular action which, in some respects, goes further than any yet recorded. He happened to procure a small Galilean telescope, or perspective glass, by Dollond, $6\frac{1}{2}$ inches focus, and $1\frac{3}{16}$ ths aperture, of which the tripod stand was so arranged as to fold up and pack into the tube. When so packed it terminated opposite the object glass in a disc of brass, in the centre of which was an aperture $\frac{1}{10}$ ths of an inch diameter, and in the centre of this the end of an iron screw, of $\frac{1}{8}$ ths of an inch. The distance of the perforated disc from the inner surface of the object glass was $\frac{1}{4}$ th of an inch. The impression of this disc and of the central pivot was delicately etched on the glass; the polished surface being disintegrated opposite the brass and iron, an *annulus*, opposite the space between

these, retaining its polish. The molecular change is extremely delicate, and can only be seen in certain inclinations to the light ; it does not seem to affect the performance of the glass. Dollond died in 1761 ; but whether made by him or his son, the instrument bears internal evidence of being very old ; and was represented as having been 40 years in the shop where it was bought. We have therefore an experiment of very long duration, and which presents these remarkable points : 1st, There is a notable distance between the radiating surfaces. 2ndly, The impression is permanently etched, and not capable of being removed by any cleaning of the surface. It would be out of place in this note to enter on the theory of this effect.

[W. R. G.]

Friday, February 12.

H.R.H. THE PRINCE CONSORT, K.G. D.C.L. F.R.S. Vice-Patron,
in the Chair.

PROFESSOR FARADAY, D.C.L. F.R.S.

Remarks on Static Induction.

THE object of the speaker was to give to the Members of the Royal Institution a simple reference to the production and nature of the static phenomena of electricity ; especially in respect of induction, into which indeed they all resolve themselves. When flannel, shell-lac, metal, and sulphur, are any two of them rubbed together they become electrified in the well-known manner ; and in such order that any one of them becomes negative to those which precede it in the list, or positive to those which follow. Thus, metal becomes negative to shell-lac, and positive to sulphur ; and as either of these substances can be employed in the investigation of the fundamental principles of induction, this difference is important in some of the methods of examining by the electrometer their temporary or permanent state. If a stick of shell-lac have a flannel cap fitted to one extremity, both being unexcited, and these, either separate or associated, be examined by the gold-leaf electrometer, they will show no signs of electricity. If the cap, grasped by the hand, be turned round on the shell-lac with friction, but left in its place, the associated substances will still show no signs of electricity. If separated, each will show a strongly excited state opposite to that of the other. If one be laid on the cap of the electrometer (the gold leaves of which were 7 inches long and $1\frac{1}{2}$ inches wide, with perfect insulation) it will show a highly excited state ; if the other be gradually brought near, and finally placed by the side of the first, all the electric signs will disappear, to reappear when the separation is again produced. The experiment presents a type case of excitation and induction. By the friction together the opposite electricities are excited ; they then exist and keep their state by mutual induction ; they are perfectly equivalent to each other, and hold their existence by this definite and relative equivalency ; for one electricity cannot exist by itself. They show no external signs of electricity whilst the forces are related only to each other, but when the two bodies in which the states are located are separated, then this relation is not exclusive, but by so much as the induction is diminished between the two sub-

stances, it is thrown in other directions ; as towards the electrometer, or the walls of the room. When one is carried into a separate room, or put into a vessel of conducting matter, then the excited bodies become independent of each other ; each has raised up an exactly equal amount of the contrary force by action terminating at a distance, according to the laws of ordinary induction. The power exerted by each excited body in this distant action may be expressed by the term, lines of force. These lines, or the force they represent, are sustained, so long as they are contained in or pass through an insulating medium. They continue, until meeting with conducting matter they evolve the contrary state to that at which they originate, and in the equivalent proportion, and so terminate the insulation ; or failing that, they continue their course outwards. If it were possible to place the excited shell-lac in the centre of an almost infinite extent of insulating medium, the lines of force would be as infinitely extended from it. If the power at any section of the whole of the lines of force could be compared with that at any other section, they would be found equal to each other ; though one section might be close to the shell-lac, and the other at an infinite distance. If there were no conducting matter at the boundaries of the insulating space above supposed, the shell-lac could not exist independently in the excited state : it would then keep its lines of force altogether turned upon the body by which it had first been excited, the induction between the two being sustained by their reciprocal action, without which electricity could neither be excited nor exist. Such are some of the consequences which follow inevitably upon the laws of static induction, combined with the law of the conservation of force.

But if this function of induction be so essential to the very existence of electricity in its developed or active state, what is its nature ? It acts through distance and across intervening bodies : how are the space and the bodies affected ? In all actions at a distance it is most important to ascertain, if possible, what occurs in the intervening medium, or the interposed space ; whether the investigation ends in the establishment of a particular process for the particular case, or the reference of the process to any more general mode of action representing all cases of distant action.

Induction acts across any insulating body, whether it be solid, fluid, or gaseous. Common air is concerned in most inductive actions, but being mobile, its particles cannot be retained in a given place, position, or state, so as to allow of close examination. Sulphur and shell-lac are excellent bodies as subjects of investigation, the more especially as their *specific inductive capacity* is about twice that of air ; and being solid bodies, their superficial or bounding particles can be thrown into a given state, yet preserved in their place to be examined with the purpose of showing what that state is. If a round plate of metal, 9 inches in diameter, be set up vertically in the air and insulated, and a like plate of good gutta-

percha raised on an insulating pillar be placed parallel to and about 9 inches from it; then, upon exciting the gutta-percha, strong induction occurs. The gutta-percha presents the inductive, the copper plate the inductive, surfaces which limit the field of induction, which field supplies an excellent place for experiment. The gutta-percha should be excited by a piece of close broad cloth, free from loose particles, and all dust, or other sources of convective effects should be avoided. Plates of sulphur, about 3 or 4 inches square, and 1 inch thick, may be employed as the inductive medium, and these having white silk loops introduced into the edges when cast, may, by the further use of white silk slings, be suspended or handled with perfect facility. Some discs of stiff paper, gilt on both sides, being attached at the edge to thin stems of shell-lac, are thus well insulated, and serve either as metallic plates or carriers.

It is almost impossible to take a block of sulphur out of paper, or from off the table without finding it electric; if, however, a small spirit-lamp flame be moved for a moment before its surface at about an inch distance, it will discharge it perfectly. Being then laid on the cap of the electrometer it will probably not cause divergence of the gold leaves; but the proof that it is in no way excited is not quite secure until a piece of uninsulated tinfoil or metal has been laid loosely on the upper surface. If there be any induction across the sulphur, due to the feeble excitement of the surfaces by opposite electricities, such a process will reveal it: a second application of the flame will remove it entirely. When a plate of sulphur is excited on one side only, its application to the electrometer does not tell at once which is the excited side. With either face upon the cap the charge will be of the same kind, but with the excited side downwards the divergence will be much, and the application of the uninsulated tinfoil to the top surface will cause a moderate diminution, which will return as the tinfoil is removed; whereas, with the excited side upwards, the first divergence of the leaves will be less, and the application of the tinfoil on the top will cause considerable diminution. The approximation of the flame towards the excited side will discharge it entirely. The application near the unexcited side will also seem partly to discharge it, for the effect on the electrometer will be greatly lessened; but the fact is, that the flame will have charged the second surface with the *contrary* electricity. When therefore the originally excited surface is laid down upon the cap of the electrometer, a diminished divergence will be obtained, and it is only by the after application of uninsulated tinfoil upon the upper surface that the full divergence due to the lower surface is obtained.

Being aware of these points, which are necessary to safe manipulation, and proceeding to work with a plate of sulphur in the field of induction before described, the following results are obtained: A piece of uncharged sulphur being placed in the induction field parallel

of course to the gutta-percha and copper-plates, and retained there, even for several minutes, provided all be dry and free from dust and small particles, when taken out and examined by the electrometer, either without or with the application of the superposed tinfoil, is found without any charge. The gilt plate carrier before described, if introduced in the same position and then withdrawn is found entirely free of charge. If the sulphur plate be in place, and then the carrier be introduced and made to touch the face of the sulphur, then separated a small space from it, and brought away and examined, it is found without any charge; and that whether applied to either one side or the other of the block of sulphur. So that any of these bodies, which may have been thrown into a polarized or peculiar condition whilst under the induction, must have lost that state entirely when removed from the induction, and have resumed their natural condition. Assuming, however, that the sulphur had become electrically polarized in the direction of the lines of induction, and that therefore whilst in the field one face was positive and the other negative, the mere touching of two or three points by the gold-leaf carrier would be utterly inefficient in bringing any sensible portion of this charge or state away; for though metal can come into *conduction contact* with the surface particles of a mass of insulating matter, and can take up the state of that surface, it is only by real contact that this can be done. Therefore the two sides of a block of sulphur were gilt by the application of gold leaf on a thin layer of varnish, and when the varnish was quite dry and hard this block was experimented with. Being introduced into the induction field for a time and then brought away, it was found free from charge on both its surfaces; being again introduced, and the carrier placed between it and either the gutta-percha or the copper-plate, but not touching these or the sulphur, the carrier when brought away showed no trace of electricity. The carrier being again introduced at the inductive or gutta-percha side, made to touch the gilt surface of the sulphur on that side, separated a little way and then brought out to be examined, gave a positive charge to the electrometer: when it was taken to the other side of the sulphur and applied in the same manner, it brought away a negative charge. Thus showing, that whilst the sulphur was under induction, the side of it towards the negative gutta-percha was in the positive state, and the side towards the positive inductive surface of copper bounding the extent of the induction field, was in the negative state. Thus the dielectric sulphur whilst under induction is in a constrained polar electrical state, from which it *instantly* falls into an indifferent or natural condition the moment the induction ceases, either by the removal of the sulphur or the gutta-percha. That this return action is due to an electrical tension *within* the mass, sustained while the act of induction continues, is evident by this, that if the carrier be applied two or three times alternately to the two faces, so as to discharge in part the electricity

they show under the induction, then on removing the sulphur from the induction field it returns, not merely to neutrality or indifference, but the surfaces assume the opposite states to what they had before; a necessary consequence of the return of the mass of inner particles to or towards their original condition.

The same result may be obtained, though not so perfectly, without the use of any coatings. Having the uncoated sulphur in its place, put the small spirit lamp between it and the copper-plate; bring up the excited gutta-percha to its place, remove the spirit-lamp flame, and then the gutta-percha, and finally, examine the sulphur: the surface towards the flame, and *that only*, will be charged—its state will be found to be positive, just like the same side of the gilt sulphur which had been touched two or three times by the carrier. During the induction, the mass of the sulphur had been polarized; the anterior face had become positive; the posterior had become negative; the flame had discharged the negative state of the latter; and then, on relieving the sulphur from the induction, the return of the polarity to the normal condition had also returned the anterior face to its proper and unchanged state, but had caused the other, which had been discharged of its temporary negative state whilst under induction, now to assume the positive condition. It would be of no use trying the flame on the other side of the sulphur plate, as then its action would be to discharge the gutta-percha, and destroy the induction altogether.

When several plates were placed in the inductive field apart from each other, subject to one common act of induction, and examined in the same manner, each was found to have the same state as the single plate described. It is well known that if several metallic plates were hung up in like manner, the same results would be obtained. From these and such experiments, the speaker took occasion to support that view of induction which he put forth twenty years ago,* which consists in viewing insulators as aggregates of particles, each of which conducts within itself, but does not conduct to its neighbours, and induction as the polarization of all those particles concerned in the electric relation of the inductive and inducteous surfaces; and stated, that as yet he had not found any facts opposed to that view. He referred to specific inductive capacity, now so singularly confirmed by researches into the action of submarine electro-telegraphic cables, as confirming these views; and also to the analogy of the tourmaline, whilst rising and falling in temperature, to a bar of solid insulating matter, passing into and out of the inductive state.

[M. F.]

* Phil. Trans., 1837.

PROFESSOR FARADAY, *on Static Induction.*

(Addition to the Report of the 12th of February.)

The inquiries made by some who wish to understand the real force of the test experiments relating to static induction, brought forward on the above date (page 470,) and their consequences in relation to the theory of induction, make me aware that it is necessary to mention certain precautions which I concluded would occur to all interested in the matter: I hope the notice I propose to give here will be sufficient. When metallic coatings or carriers are employed for the purpose of obtaining a knowledge of the state of a layer of insulating particles, as those forming the surface of a plate of sulphur, it is very necessary that they should exist in a plane perpendicular to the lines of the inductive force, and in a field of action where the lines of force are *sensibly equal*. Hence the importance of the dimensions given in the description of the apparatus at page 472 of the report of the evening, when the inductive surfaces are described as 9 inches in diameter, and 9 inches apart. The inductive surface there mentioned is a plane: a ball cannot properly be used for this purpose: for the lines of inductive force originating at it cannot then be perpendicular to the layer of gold-leaf forming the coating of the sulphur. The consequence would be that this layer of gold being virtually extended along the lines of inductive force, *i.e.* having parts nearer to and parts more distant from the inductric, will be polarized according to well-known electrical actions, will have opposite states at those parts, will show these states by a carrier, and will give results not belonging merely to insulating particles in a section across the lines, but chiefly to united conducting particles in a section oblique to or along the lines.

The carrier itself must be perfectly insulated the whole time, or else a case of induction, not including the sulphur, and entirely different to that set out with is established. It must not even extend by elongation into parts of the field of induction where the force differs in degree: or else errors of the same kind as those described with the ball inductric will occur. It should also be so used as to receive no charge by convection. When introduced between the inductric and the sulphur, it is very apt, if the charge be high, or if particles adhere to the inductric, to receive a charge. This is easily tested by introducing the carrier into its place, abstaining from touching the gold-leaf, withdrawing the carrier, and examining it: it is not until this can be done without bringing away any charge that the carrier should be employed to touch the gold-leaf surface, and bring away the indication of its electrical state.

As before said, if when the state of matters is perfect, and no convection interferes, the gilt sulphur be put into its place, left

there for a short time, and brought away again, it will be found without *any* charge either of the gold-leaf coating or the sulphur. If it be put into place, the coating next the inductric be uninsulated for a moment only, and the plate brought away, that coating will then appear positive. If it be put into place and the further gold-leaf be uninsulated for a moment, that coating when the plate is brought away will be found negative. These are all well known results, and will always appear if convection and other sources of error be avoided.

22nd March, 1858.

M. F.

Friday, May 28.

WILLIAM ROBERT GROVE, Esq. M.A. Q.C. F.R.S. Vice-President,
in the Chair.

PROFESSOR E. FRANKLAND, F.R.S. F.C.S.

LECTURER ON CHEMISTRY AT ST. BARTHOLOMEW'S HOSPITAL.

*On the Production of Organic Bodies without the agency of
Vitality.*

THE earlier researches of chemists brought them into contact with two classes of bodies, distinguished from each other by well-marked and obvious peculiarities. One of them was met with in the inanimate or mineral kingdom, the various materials of which were distinguished by their comparative stability or resistance to change, and by the facility with which the greater number of them could be artificially produced from the elementary bodies composing them. The other class of bodies was found exclusively in the animate portion of creation, or was directly derived from the productions of the organs of plants and of animals: these compounds were distinguished by their proneness to undergo change, and by the impossibility of producing them by artificial means. By no processes then known to chemists, could the elements composing these latter bodies be made to unite, so as to produce compounds, either identical with, or analogous to, the substances generated by the organs of plants and of animals. These substances were consequently, from their origin, termed *organic bodies* or *organic compounds*. They were regarded as dependent for their origin, upon the influence of that aggregate of conditions sometimes called *vital force*; and it was generally believed, that we should never succeed in producing these bodies artificially, until we could form, and endow with vitality, the organs from which they were derived.

Such was the state of knowledge and opinion until the year 1828, when Wöhler succeeded in artificially producing *urea*, a body which had up to that time been known only as a product of the animal organism.* This discovery was followed many years later by the artificial formation of acetic acid, which was produced by Kolbe from a mixture of protochloride of carbon, water, and chlorine exposed to sunlight, the chloracetic acid thus obtained

* The artificial formation of urea from cyanate of ammonia was exhibited under the influence of polarised electric light.

being afterwards converted into acetic acid by an amalgam of potassium. The subsequent production of methyl by the same chemist from acetic acid, added one of the organic radicals to the list of compounds producible from their elements. Although little further progress was made for several years in this department of chemical research, yet the artificial production of urea and acetic acid, together with their derivatives, completely broke down the barrier between so-called "organic" and "in-organic" bodies; and although the name "organic" was still retained for the class of bodies to which it had previously been assigned, it was now obviously no longer strictly applicable.

The recent ingenious researches of M. Berthelot have greatly extended this branch of chemical enquiry, and have, in a most important degree, increased the number of bodies capable of artificial formation. The production of chloride of methyl and the members of the olefant gas family up to amylene ($C_{10}H_{10}$) furnish us with the whole series of alcohols and their derivatives, from amylic alcohol downwards. Phenyl alcohol and naphthaline, both artificially produced by Berthelot, yield a host of interesting bodies; whilst phenylcarbamic acid enables us to step from the phenylic to the salicylic group, since, when treated with hyponitrous acid, it yields salicylic acid. Lastly, M. Berthelot has succeeded in artificially forming glycerine, the basis of animal and vegetable oils and fats, and also in forming grape sugar; the latter however is obtained by the contact of glycerine with putrifying animal matter, and consequently cannot be said to be produced altogether without the agency of vitality; although the putrifying organic matter contributes none of its constituents to the new compound, and does not undergo any appreciable change in weight or appearance during the process. These substances yield such a numerous class of derivatives that upwards of 700 distinct organic compounds can now be produced from their elements without the agency of vitality.

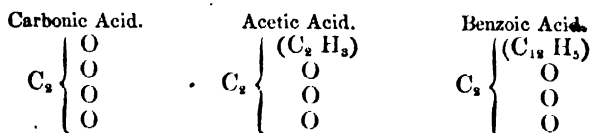
The processes employed for the artificial production of these bodies, though deeply interesting, present, with one or two exceptions, little or no analogy to the natural mode by which organic compounds are formed in the tissues of plants; but the speaker endeavoured to show, that a close attention to the nature of the inorganic materials assimilated by the vegetable kingdom, and their relations to the more important organic compounds derived from plants, leads to the belief, that such compounds can be successfully produced by processes strictly analogous to those employed by nature. He contended that the constitution of the so-called organo-metallic bodies, in which the production of complex organic compounds from inorganic ones by the replacement of elements by organic groups, can be so clearly traced, afforded a valuable clue to the formation of organic bodies in general, and led directly to the conclusion, that if the organic compounds of the metals be formed upon the model of the oxides of the respective metals, the organic

compounds of carbon (that is, all organic compounds) are formed upon the model of the oxides of carbon.

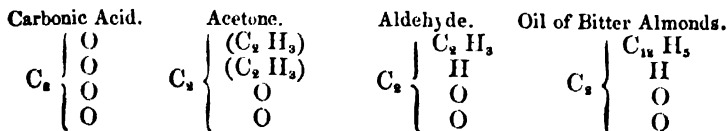
It has long been known, that with slight and unimportant exceptions, the only materials employed by nature in the construction of the most complex organic compounds, are carbonic acid, water, ammonia, and nitric acid. The fact that a vast number of organic compounds are cast in the molecular mould of water, has been proved by the ingenious researches of Williamson and Gerhardt; whilst the wondrous fertility of the ammonia model has been amply demonstrated by the labours of Hofmann and Wurtz. It would also not be difficult, to prove the claim of nitric acid to be considered as a third model, upon which a number of other organic compounds are built up; but it was necessary to confine attention on the present occasion to the consideration of carbonic acid only, as a model upon which a very large number of organic bodies are formed.

Guided by the constitution above referred to, of the organo-metallic bodies, and bearing in mind the replacibility of the oxygen in water and binoxide of nitrogen, and the chlorine in terchloride of phosphorus, by organic radicals; Professor Kolbe and the speaker were led to the following hypothesis regarding the constitution of several important classes of organic compounds.

1. The replacement of one atom of oxygen in carbonic acid by hydrogen, or its homologues, produces an organic acid, either of the fatty or of the aromatic series, thus:—



2. The like replacement of two atoms of oxygen in carbonic acid, produces either a ketone, or an aldehyde, thus:—



3. The like replacement of three atoms of oxygen in carbonic acid, produces an ether, thus:—



4. The like replacement of all the atoms of oxygen in carbonic acid, produces a radical, a hydride of a radical, or a double radical, thus :—

Carbonic Acid.	Ethyl.	Hydride of Methyl. (Fire damp.)	Methyl-ethyl.
$C_2 \left\{ \begin{array}{l} O \\ O \\ O \\ O \end{array} \right.$	$C_2 \left\{ \begin{array}{l} C_2 H_3 \\ H \\ H \\ C_2 H_3 \end{array} \right.$	$C_2 \left\{ \begin{array}{l} H \\ H \\ H \\ H \end{array} \right.$	$C_2 \left\{ \begin{array}{l} C_2 H_3 \\ H \\ H \\ C_2 H_3 \end{array} \right.$

The authors of this hypothesis now attempted to verify it by direct experiment. They endeavoured to avail themselves of the powerful affinities of zinc-ethyl, in order to effect the substitution of oxygen in carbonic acid, and sulphur in bisulphide of carbon, by ethyl; these attempts were, however, at best only partially successful; the re-agent, the zinc-ethyl, was not sufficiently powerful to rival the action of plants in the decomposition of carbonic acid; and its effects upon bisulphide of carbon resulted in the production of a number of organic bodies containing sulphur; and although one of these appeared to have the formula of sulphopropionic acid ($C_3 H_5 S_2 + 2H$), yet its complete separation and purification presented such difficulties, that it would have been hazardous to rely upon it as a proof of the correctness of their hypothesis. In short, the verification of these views was not permitted to their authors, but was reserved for Mr. Wanklyn, who, in his newly-discovered sodium and potassium compounds of the organic radicals, came into possession of re-agents, which enabled him at once to effect the desired substitutions. His memoir on the production of propionic acid by the action of sodium-ethyl upon carbonic acid,* which has just been communicated to the Chemical Society, proves the first proposition of a hypothesis, which considerably simplifies our views of the molecular structure of organic bodies, and which, if proved to be throughout correct, cannot fail to enable us greatly to increase the number of organic compounds capable of being procured from their elements without the intervention of vitality.

The speaker then referred to the following list of important organic bodies, selected from the large number above spoken of, as being capable of artificial formation from their elements :—

Name.	Formula.
Oxalic Acid	$(C_2 O_3, H O)_2$
Hydrocyanic Acid	$C_2 N, H$
Light Carburetted Hydrogen	$C_2 H_4$
Urea	$C_2 N_2 H_4 O_2$
Formic Acid (Acid of Ants)	$C_2 H O_2, H O$

* This conversion of carbonic acid into propionic acid, was experimentally demonstrated, and the remarkable properties of sodium-ethyl and potassium-ethyl were also exhibited.

Chloroform	$C_3 H Cl_3$.
Acetic Acid	$C_4 H_8 O_2, H O$.
Alcohol	$C_4 H_8 O, H O$.
Ether	$(C_4 H_8 O)_2$.
Olefiant Gas	$C_4 H_4$.
Acetic Ether	$C_4 H_8 O, C_4 H_8 O_2$.
Oil of Garlic	$(C_6 H_8 S)_2$.
Oil of Mustard	$C_6 H_8 S, C_2 N S$.
Glycerine	$C_6 H_8 O_6$.
Butyric Acid	$C_8 H_7 O_2, H O$.
Pine Apple flavour (Butyric Ether)	$C_8 H_7 O_2, C_4 H_8 O$.
Succinic Acid	$C_8 H_4 O_6, 2 H O$.
Valerianic Acid	$C_{10} H_9 O_3, H O$.
Pear flavour (Acetate of Amyl)	$C_4 H_8 O_2, C_{10} H_{11} O$.
Apple flavour (Valerianate of Amyl)	$C_{10} H_9 O_3, C_{10} H_{11} O$.
Lactic Acid	$C_{12} H_{12} O_{12}$.
Grape Sugar ?	$C_{12} H_{12} O_{12}$.
Caproic Acid	$C_{12} H_{11} O_3, H O$.
Benzole	$C_{12} H_6$.
Nitrobenzole	$C_{12} H_5 N O_2$.
Aniline	$N (C_{12} H_5)_2$.
Phenyl Alcohol (Creosote)	$C_{12} H_5 O, H O$.
Picric Acid	$C_{12} H_2 (N O_2)_3 O, H O$.
Salicylic Acid	$C_{14} H_5 O_5, H O$.
Salicylate of Methyl (Oil of Wintergreen)	$C_{14} H_5 O_5, C_2 H_3 O$.
Naphthaline	$C_{20} H_8$.

The artificial formation of urea, lactic acid, and caproic acid, is interesting in connection with certain functions of the animal economy. Pine-apple oil, pear oil, and apple oil, are instances of the artificial production of the delicate flavours of fruit, whilst oil of wintergreen and nitrobenzole are like examples of the formation of esteemed perfumes. But of all the bodies hitherto thus produced, alcohol, glycerine, and sugar, are undoubtedly the most deeply interesting, owing to the part they take in the nutrition of animals : they prove to us the possibility of producing, without vegetation or any vital intervention, an important part of the food of man. Should the chemist also succeed in forming artificially the nitrogenous constituents of food, without which life cannot be maintained, it would then be possible for a man, placed upon a barren rock, and furnished with the necessary apparatus and inorganic materials, to support life entirely without either animal or vegetable food. No one of these nitrogenous constituents has however yet been artificially produced, and the absence of all clue to their rational constitution forms at present a formidable barrier to their non-vital formation.

It would be difficult to conclude a subject like the present, without any notice of the considerations which naturally suggest them-

selves, regarding the possibility of economically replacing natural processes by artificial ones in the formation of organic compounds. At present, the possibility of doing this only attains to probability in the case of rare and exceptional products of animal and vegetable life. Thus valerianic acid, which for a long time was extracted from the root of the *Valeriana officinalis*, could now probably be more cheaply prepared from its elements ; but a still cheaper source of this acid has been in the meantime discovered, viz. the oxidation of amylic alcohol, a waste product formed in the manufacture of spirit of wine, and obtainable at such a moderate cost as to prevent, in an economical point of view, the successful production either of amylic alcohol or valerianic acid by any artificial and exclusively non-vital processes at present known. It is also highly probable, that if we could produce artificially such bodies as quinine and the rare alkaloids, or alizarine and similar powerful and valuable organic colouring matters, we should be able to compete with organic life in the formation of these bodies ; nevertheless, the discovery of the processes of artificial formation would doubtless be preceded by a knowledge of methods, by which such rare bodies could be produced from more abundant, and consequently cheaper forms of vegetable or animal matter ; and it is therefore exceedingly improbable, that any purely non-vital process will be successfully, and at the same time economically employed for the manufacture even of such rare and valuable vital products. Such being the economical bearings of the case with regard to the rare and exceptional educts of vitality, when we turn to consider the great staple products of the animal and vegetable kingdoms, the hope of rivalling natural processes becomes faint indeed. By no processes at present known could we produce sugar, glycerine, or alcohol from their elements, at one hundred times their present cost, as obtained through the agency of vitality. But, although our present prospects of rivalling vital processes in the economical production of staple organic compounds, such as those constituting the food of man, are so exceedingly slight, yet it would be rash to pronounce their ultimate realization impossible. It must be remembered, that this branch of chemistry is as yet in its merest infancy, and that it has hitherto attracted the attention of few minds ; and further, that many analogous substitutions of artificial for natural processes have been achieved. Thus, under certain circumstances, we find it less economical to propel our ships by the force of the wind, and our carriages by animal power, than to employ steam power for these purposes. We do not find it desirable to wait for the bleaching of our calicoes by the sun's rays ; and even the grinding of corn is no longer entirely confided to wind and water power.

In such cases, where contemporaneous natural agencies have been superseded, we have almost invariably drawn upon that grand store of force collected by the plants of bygone ages, and conserved in

our coal fields. It is the solar heat of a past epoch that furnishes the power which we now utilise in our steam-engines. One important element in cheap production is *time*, and it is precisely in regard to this element, that we economically supersede, in the above instances, the contemporary resources of nature. Now time is also an important element in the natural production of food; and although it is true, that the amount of labour required for the production of a given weight of food is not considerable, yet it is nevertheless true that this weight requires a whole year for its production. By the vital process of producing food we can only have one harvest in each year. But if we were able to form that food from its elements without vital agency, there would be nothing to prevent us from obtaining a harvest every week; and thus we might, in the production of food, supersede the present vital agencies of nature, as we have already done in other cases, by laying under contribution the accumulated forces of past ages, which would thus enable us to obtain in a small manufactory, and in a few days, effects which can be realized from present natural agencies, only when they are exerted upon vast areas of land, and through considerable periods of time.

[E. F.]

Friday, January 28.

SIR HENRY HOLLAND, BART. M.D. F.R.S. in the Chair.

W. R. GROVE, ESQ. Q.C. F.R.S. *V.P.R.I.*

On the Electrical Discharge, and its Stratified Appearance in Rarefied Media.

FEW subjects of physical investigation possess greater interest than the electrical discharge ; its brilliant effects and mysterious characteristics offer powerful stimuli to curiosity and enquiry. The speaker proposed first shortly to state the extent of knowledge we possess respecting it ; then to pass to certain peculiar phenomena first discovered by him in 1852, and subsequently experimented on by others, and most elaborately by Mr. Cassiot ; and then to offer an opinion as to their cause or rationale.

The best mode of examining and attempting to explain the electrical discharge is to compare it with its nearest analogue flame, to which one form of the discharge, viz. the Voltaic arc, has much seeming resemblance. The flame of a common candle results, as is well known, from the chemical combination of carbon and hydrogen with the oxygen of the air ; and the combustion is most brilliant where the heated gases and particles are in proximity to the oxygen. It forms a hollow cone, as the oxygen of the air, being consumed or combined into water and carbonic acid at the exterior portion, cannot reach the interior : the course of the currents of heated air, and the particular form of this hollow cone of flame, are beautifully shown by the refraction it produces on a more brilliant light, such as that of the electric lamp ; the flame issues from a single nucleus, the wick ; and the amount of heat produced is definite for a definite amount of chemical combination.

In the Voltaic arc there are two points or *foci* ; the polar terminals there undergo a change, but not a consumption equivalent or nearly so to the heat and light produced ; but if the consumption of the zinc or the quantity of it combined with oxygen in the cells of the battery be compared with the amount of heat generated in the arc, plus that in the cells of the battery and conducting wires, the same amount of total heat will be found to be developed as if the same quantity of zinc were simply burned in oxygen.

By subdividing more and more the plates of the voltaic battery and proportionately increasing their number, we gradually increase the length and diminish the volume of the arc until at length we arrive, as in the voltaic columns of De Luc and Zamboni, at the electric spark.

The spark from a Ruhmkorff coil was projected on a screen by the

electric lamp, and the impression contrasted with that of the flame of a candle; in the former two cones are seen to issue from the terminals instead of the single one of the latter, one being more powerful, and overcoming or beating back the other; and this effect is reversed as the direction of the current is reversed.

In all cases hitherto observed there is a dispersion or projection of a portion of the terminals; this takes place in all forms of electric disruptive discharge, whatever be the materials of which the terminals are composed. In the voltaic arc there is a transmission of matter, principally from the positive, which is the more intensely heated, to the negative terminal; in the spark from the Ruhmkorff coil the dispersion is principally, and in some cases appears to be entirely, from the negative terminal, while this is now the more intensely heated.

In addition to this, there is generally, but not always, a change produced in the medium across which the discharge passes; compound liquids, vapours, and gases are decomposed, and even elementary gases are allotropically changed. There is also a polar condition of the electrical discharge, which produces the converse chemical effects at each pole—effects described by Mr. Grove in a paper in the Philosophical Transactions for 1852, and subsequently shown at an evening meeting of this Institution.

Gases offer a powerful resistance to the passage of the discharge, but this resistance is diminished as the gases are rarefied; and a discharge which would not pass across a space of half an inch in air of the ordinary density will pass through several feet in highly attenuated air.

In experimenting on the passage of the discharge through the vapour of phosphorus in 1852, Mr. Grove observed for the first time that the discharge was traversed by a number of dark bands or striæ. At first he was disposed to attribute this phenomenon to some peculiarity of the medium; but on trying good *vacua* of other vapours and gases, he found the striæ were in all cases visible, and seemed to depend on the degree of rarefaction of the gas. Many subsequent experiments have been made by himself and others on the subject, and more particularly by Mr. Gassiot; and the extent of knowledge we have acquired upon this still mysterious phenomenon was now discussed and illustrated.

In the vapour of phosphorus the striæ generally exhibit themselves like narrow ruled lines, about 0.05 inch diameter, transverse to the line of discharge; but with certain precautions they become wider and assume a conical form, somewhat resembling the whalebone snakes made as a toy for children. Mr. Gassiot has used most carefully prepared Torricellian *vacua*, and has also, in conjunction with Dr. Frankland, obtained excellent *vacua*, by filling tubes containing sticks of caustic potass with carbonic acid, exhausting them by the air-pump, and allowing the residual gas to be absorbed by the potass.

The following is a summary of the effects produced by the electric discharge through these *vacua*.

If the vacuum be equal to that generally obtained by an ordinary

air-pump, no stratifications are perceptible; a diffused lambent light fills the tube: in a tube in which the rarefaction is carried a step further, narrow striæ are perceptible, like those first described in the phosphorus vapour experiment. A step further in rarefaction increases the breadth of the bands; next we get the conical or cup-shaped form; and then, the rarefaction being still higher, we get a series of luminous cylinders of an inch or so in depth, with narrow divisions between them. Lastly, with the best vacua which have been obtained, there is neither discharge, light, or conduction.* The fact of non-conduction by a very good Torricellian vacuum was first noticed by Walsh, subsequently carefully experimented on by Morgan (Philosophical Transactions, 1785), and subsequently by Davy (1822); the latter did not obtain an entire non-conduction, but a considerable diminution both of light and conducting power.

From these repeated experiments it may fairly be considered as proved, that *in vacuo*, or in media rarefied beyond a certain point, electricity will not be conducted, or more correctly speaking, transmitted; an extremely important result in its bearing on the theories of electricity.

The gradual widening of the strata, as the rarefaction proceeds, is in favour of the phenomena of stratification being due to mechanical impulses of the attenuated medium, and appears to support the following rationale of the phenomenon given by Mr. Grove; who does not advance it as conclusive, but only as an approximation to a theory to be sifted by further experiments. When the battery contact is broken, there is generated the well-known induced current in the secondary wire in the same direction as the original battery current, to which secondary current the brilliant effects of the Ruhmkorff coil are due: but in addition to this current in the secondary wire, there is also a secondary current in the primary wire, flowing in the same direction; the induction spark, at the moment following the disruption of contact, completing the circuit of the primary, and thus allowing the secondary current to pass. This secondary current in the primary wire produces in its turn another secondary, or what may be termed a tertiary, current in the secondary wire, in an opposite direction to the secondary current. There are thus, almost synchronously, two currents in opposite directions in the secondary wire; these, by causing a conflict or irregular action on the rarefied medium, would give rise to waves or pulsations, and might well account for the stratified appearance. The experimental evidence in favour of this view is as follows: when a single break of battery contact is made by drawing a stout copper wire over another wire, the striæ do not invariably appear

* The production of vacua by carbonic acid, and the increasing breadth of the stratifications with increased rarefaction, was communicated by Mr. Gassiot in a paper, read to the Royal Society, Jan. 13, 1859. I incline to think that oxygen-hydrogen gas, with potash, might give a better vacuum than carbonic acid, as the last residual portions of the gas would be slowly combined by the discharge, and the water so formed absorbed by the potash.—W. K. G.

in the rarefied medium through which the current of the secondary wire passes. This would be accounted for on the above theory by supposing that in some cases of disruption the induced spark passes across immediately on disruption, and thus completes the circuit for the secondary current in the primary wire; while in other cases, either from want of sufficient intensity, or from the mode or velocity with which contact is broken, or from the oxidation of the points where contact is broken, there is no induced spark by which the current can pass: in the former case there would be a tertiary current in the secondary wire, and therefore striæ; in the latter there would be none.

But the following experiment is more strongly in favour of the theory. It is obvious that the secondary must be more powerful than the tertiary current. Now supposing an obstacle or resistance placed in the secondary circuit, which the secondary current can overcome but the tertiary cannot, we ought by the theory to get no striæ. If an interruption be made in the secondary current in addition to that formed by the rarefied medium, and this interruption be made of the full extent which the spark will pass, there are, as a general rule, no striæ in the rarefied medium, while the same vacuum tube shows the striæ well if there be no such break or interruption. The experiment was shown by a large vacuum cylinder (16 inches by 4) of Mr. Gassiot, and his micrometer electrometer; this tube showed numerous broad and perfectly distinct bands when the points of the micrometer were in contact; but when they were separated to the fullest extent that would allow sparks to pass, not the slightest symptom of bands or striæ were perceptible, the whole cylinder was filled with an uniform lambent flame. With a spark from the prime conductor of the electrical machine, the striæ do not appear in tubes which show them well with the Ruhmkorff coil; occasionally, and in rare instances, striæ may be seen with sparks from the electrical machine, but, not as far as Mr. Grove has observed, when the spark is unquestionably single. All this is in favour of the theory given above; but without regarding that as conclusive or as a proved *rationale*, it is clearly demonstrated by the above experiments, that the identical vacuum tubes which show the striæ with certain modes of producing the discharge, do not show them with other modes, and that therefore the striæ are not a necessary condition of the discharge itself in highly attenuated media, but depend upon the mode of its production.

The study of the electrical discharge *in vacuo* is of the utmost importance in reference to the theories of electricity, and probably will assist much towards the proper conception of other modes of force, or, as they are termed, *imponderables*, heat, light, &c.

The experiments of Walsh and Morgan, corroborated as they now are by that of Mr. Gassiot, show, that although the transmission of electricity across gaseous media is aided by rarefaction of the medium up to a certain degree, yet that a degree of attenuation may be reached at which the transmission ceases, at all events for a given dis-

tance between the terminals and given intensity of electrical charge. Whether having arrived at this point a reduction of the space to be traversed, or an increase of intensity in electricity, or both, would again enable the electricity to pass, is not quite clear, though there is reason to believe that it would, and the increased intensity of electricity would probably be again stopped by a further improvement in the vacuum, and so on. But the experiments go far to prove that ordinary matter is requisite for the transmission of electricity, and that if space could exist void of matter, then there would be no electricity : thus supporting the views advocated by Mr. Grove and some others, that electricity is an affection or mode of motion of ordinary matter.

The non-transmission of electricity by very highly attenuated gas may also afford much assistance to the theory of the aurora borealis, a phenomenon, the appearance of which, the regions where it is seen, its effect on the magnet, and other considerations, have led to the universal belief that it is electrical.

The experimental result that a certain degree of attenuation of air forms a good conductor, or easy path for the electrical force, while either a greater or a less degree of density offers more resistance, and this increasing towards either extremity of density or rarefaction, show, that if there be currents of electricity circulating to or from the polar regions of the earth, the return of which, as is generally believed, gives rise to the beautiful phenomena of the aurora borealis or australis, the height where this transit of electricity takes place would be just that at which the density of the air is such as to render it the best conductor. By careful measurement of the degree of attenuation requisite to enable the electrical discharge to pass with the greatest facility in our laboratory experiments, we may approximatively estimate the degree of rarefaction of the atmosphere at the height where the aurora borealis exists. By these means we get a mode of estimating the height of the aurora by ascertaining, from the decrement of density in the atmosphere in proportion to its distance from the earth, at what elevation the best conducting state, or that similar to our best conducting vacuum tubes would be found, or conversely, by ascertaining the height of the aurora by parallax measurements, we may ascertain the ratio of decrement in the density of the atmosphere. Thus by our cabinet experiments, light may be thrown on the grand phenomena of the universe, and the great questions of the divisibility of matter, whether there is a limit to its expansibility, whether there is a fourth state of attenuation beyond the recognised states of solid, liquid, and gaseous, as Newton seemed to suspect, (30th query to the Optics,) and whether the imponderables are specific affections of matter in a peculiar state, or of highly attenuated gaseous matter, may be elucidated. The manageable character of the electrical discharge, and the various phenomena it exhibits when matter is subjected to its influence in all those varied states which we are enabled, by experiment, to reduce it, can hardly fail to afford new and valuable information on these abstruse and most interesting enquiries.

[W. R. G.]

Friday, February 25, 1859.

H. R. H. THE PRINCE CONSORT, K. G. D. C. L. F. R. S.
Vice-Patron, in the Chair.

PROFESSOR FARADAY, D. C. L. F. R. S.

On Schönbein's Ozone and Antozone.

OZONE has already been before the members of the Royal Institution on two occasions: on the 13th June 1851, when Schönbein's early views of it were given, and on the 10th June 1853, when the results of MM. Frémy and E. Becquerel, obtained by passing the electric spark through dry oxygen, were described; and also the opinion of Schönbein respecting the entrance of ozone as such (and not as simple oxygen) into combination. Since then, Schönbein has been led to the belief that oxygen can exist in a third state, as far removed by its properties from ordinary oxygen in the one direction as ozone is in the other; and therefore, in a certain sense antagonistic to ozone. This substance he names *antozone*, and believes that it also enters into combination, retaining, for the time, its special properties. Hence there is not merely ozone and antozone, but also ozonide and antozonide compounds. Thus, permanganic acid, chromic acid, peroxides of manganese, lead, cobalt, nickel, bismuth, silver, &c., form a list of bodies containing more or less of ozone in combination; and the characters of ozone, and of these bodies because of the ozone in them, is that they are electro-negative to the antozonides, *i.e.* as copper to zinc; they evolve chlorine from chlorides; they cannot generate peroxide of hydrogen; and they render blue the precipitated tincture of guaiacum. On the other hand, oxywater and the peroxides of potassium, sodium, barium, strontium, and calcium, form a list of substances containing antozone. These bodies are electro-positive to the former; they cannot evolve chlorine from hydro-chloric acid, or the chlorides; they evolve the peroxide of hydrogen when treated either by oxy-acids or even the hydro-chloric acid, and they not only do not render blue the white precipitated guaiacum, but they restore that which has been rendered blue by ozone to the white or colourless condition. Now when two ozonides or two antozonides are put together, with the addition of water or an indifferent acid, they mingle but do not act on each other; but if one body from each list be associated in like manner, they mutually act, oxygen is evolved from both, and ordinary oxygen is set free; or rather, as Schönbein believes, ozone separates from one body, and antozone from the other; and these uniting produce the

intermediate or neutral oxygen. Thenard, who discovered the peroxide of hydrogen, showed that the peroxide of silver, when brought into contact with it, not only caused the separation of part of the oxygen of the fluid, but also itself lost oxygen, that element leaving both bodies and appearing in the gaseous state. This experiment, with others of a like nature, and many new ones, were referred to and made in illustration of Schönbein's views. As to the independent existence of oxygen in these two new and antithetical states, ozone has been so obtained, *i.e.* out of combination, and independent of any other body; but antiozone has not as yet afforded this proof of its possible separate condition. Oxywater is the compound in which it seems nearest to a free condition. As Schönbein's view includes the idea that oxygen in these two states can retain their peculiar properties when out of combination, and have them conferred otherwise than by combination, and as ozone does fulfil these conditions and does exist in the independent state, so it is important that antiozone should be pursued by experiment until it gives a like result.

In relation to this subject the view of Mr. Brodie should be referred to, respecting the condition of certain elements at the *moment* of chemical change, on which he published a paper in the Phil. Trans. for 1850, p. 759, and another in the Chemical Society's Journal in 1855. He assumed oxygen as capable of existing in two states; the particles being polarized to each other by the action of associated particles, and for the moment in the relation of oxygen and hydrogen to each other; he also made many numerical experiments for the purpose of obtaining the equivalent action of the oxygens assumed to be in these opposed polar states.

[M. F.]

Friday, March 11, 1859.

CHARLES WHEATSTONE, Esq. F.R.S. Vice-President, in the Chair.

WILLIAM ODLING, M.B.

SECRETARY TO THE CHEMICAL SOCIETY.

On Magnesium, Calcium, Lithium, and their Congeners.

THE majority of the metals known at the beginning of the present century were observed to occur naturally in the earthy or oxidised state. The alkalis and earths proper, from their many analogies to the metal-yielding earths, were long suspected to be the oxides of certain unknown metals, whose tendencies to maintain the oxidised condition were stronger than those of any metals which had up to that time been isolated. This conception was first verified by Sir Humphrey Davy in 1807, and has since been abundantly realized. From their characteristic property of neutralizing acids to form salts, the earths and alkalis received the name of bases, and the metals eventually extracted from them became known as basic or basylous metals. Some of these metals, particularly those obtained from magnesia, lime, and lithia, have only of late been procured in quantities sufficient to allow of a demonstration of their properties.

The highly basylous and the commercial metals are alike obtained by three principal processes, namely electrolysis, precipitation by means of another metal, and reduction by charcoal at a red or white heat.

a. Electrolysis.—Very many metallic compounds, when submitted to the action of a galvanic current, are decomposed, with a deposition of metal upon the negative pole of the battery. Although a cheap electrolytic process has been devised for the extraction of copper from its ores, yet, altogether, electrolysis is too expensive to admit of employment for the mere separation of the commercial metals. It is, however, largely applied in the fine and useful arts for the production of different metals in certain required forms, as in the well-known operations of electrotyping, electroplating, &c. The liquid state, which is an essential condition for electrolysis, is usually obtained by dissolving the metallic compound in water; but for procuring the highly basylous metals, this means does not succeed, inasmuch as these metals cannot exist in contact with water. The basylous metals seem indeed to be deposited upon the negative pole; but, simultaneously with their production, they are acted upon by the water of the solution, and thereby converted into the state of caustic alkali. The liquid condition is consequently attained by means of fusion. Certain salts of the basylous

metals, usually the chlorides, from their ready fusibility, are melted in suitable crucibles, and then submitted to electrolysis; this is the only mode by which the metals calcium and lithium are obtainable.

β. *Precipitation of one metal by another.*—This process is largely employed on a manufacturing scale. At the mines of Freyburg, metallic silver is obtained by agitating chloride of silver with scrap iron. The iron enters into combination with the chlorine, and turns out the silver. At the Royal Mint, silver is obtained by immersing plates of copper into solution of sulphate of silver. At the Cornish mines, considerable quantities of copper are annually obtained by immersing pieces of iron in solutions of copper. These processes are performed in the wet way. As an example of the dry way, we may adduce the metal antimony, which is made commercially by fusing sulphide of antimony with scrap iron. The iron turns out the antimony and unites with the sulphur. Similarly the metals aluminium and magnesium are prepared by fusing their respective chlorides with metallic sodium; the sodium unites with the chlorine and turns out the aluminium or magnesium. To obtain metals by this process of substitution, it is ordinarily necessary that the metal used to expel another must be more basylous than the metal expelled; hence it is that sodium is required for the production of magnesium. With the exception of potassium, which is much more expensive, sodium is the most basylous of the metals; it even serves to displace the quasi-metallic grouping of hydrogen and nitrogen, known as ammonium. Amalgam of sodium, introduced into a solution of chloride of ammonium, forms chloride of sodium and amalgam of ammonium. But these most highly basylous metals, potassium and sodium, afford remarkable exceptions to the law that basylous metals replace less basylous metals. Thus, although when sodium is heated with hydrate of iron, the sodium expels the iron, as might be anticipated, yet when hydrate of sodium and iron borings are heated together, a reverse action takes place, and the iron turns out the sodium, as in Gay-Lussac's process for the production of that metal. This reciprocity of results is only an extreme instance of a tolerably general law. In a similar manner, though mercury displaces silver from argentic nitrate, yet silver displaces mercury from mercurous nitrate. Though copper displaces silver from argentic sulphate, yet silver displaces copper from cupric sulphate. Though cadmium displaces copper from cupric chloride, yet copper displaces cadmium from cadmic chloride, &c. &c. Some of these results appear to depend on Brodie's law of homogeneous affinity: thus, when cadmium is deposited upon copper, we have the reaction,



Reduction by charcoal.—This is the most usual means adopted for the production of metals on a manufacturing scale. Brunner's process for obtaining potassium and sodium is an exact counterpart of

the commercial process for obtaining zinc; in each case the metal is evolved in the gaseous state, or distilled, from a heated mixture of its carbonate with charcoal. The same reciprocity exists between sodium and charcoal as between sodium and iron. Thus carbon decomposes oxide and carbonate of sodium to form carbonic oxide or anhydride (acid). But sodium heated in carbonic oxide or anhydride liberates carbon, and forms oxide or carbonate of sodium. Indeed chemical re-actions are not absolute but conditional; under reversed condition we obtain reversed results.

Magnesium.—Chloride of magnesium is the source from which the metal is usually obtained. Becquerel succeeded in procuring octahedral crystals of magnesium by the electrolysis of a solution of chloride of magnesium. But the metal is preferably obtained by electrolysis of the fused salt. Matthiessen employs a common tobacco-pipe for the purpose: the bowl is filled with fused chloride of magnesium, or rather with a mixture of chloride of magnesium and chloride of potassium, which is more easily prepared than the pure salt. The negative pole, to which the magnesium attaches itself, consists of an iron wire passing through the pipe-stem. The positive pole consists of a pointed piece of gas-carbon dipping into the fused mixture of salts. Magnesium is however obtained most abundantly by heating its chloride with metallic sodium, as before referred to. The sodium turns out the magnesium, which collects in globules that may be melted together under a flux of low density. Magnesium is a solid metal of a silver-white colour. Its appearance contrasts favourably with that of aluminium, which has a decided bluish tinge. The freshly cut surface of metal is highly lustrous; it does not tarnish in dry air, and acquires only a film of oxide in moist air. Magnesium is but very slowly acted upon by pure cold water. It decomposes boiling water somewhat rapidly with evolution of hydrogen. Its specific gravity is 1.75. It is about one and a half times lighter than aluminium, and is indeed the lightest of all metals that are permanent in the air. One cubic inch of platinum balances $12\frac{1}{4}$ cubic inches of magnesium, and only $8\frac{1}{4}$ cubic inches of aluminium. At ordinary temperatures magnesium is somewhat brittle, and may be readily cut or filed. It is neither very malleable nor very ductile; but, at an increased temperature, may be hammered into plates, and drawn or rather pressed into wire by Matthiessen's process. The metal is placed in a small hollow steel cylinder, having a hole drilled in its anterior face. Through this hole the metal is forced in the form of wire, by means of a press, acting through the intervention of an iron piston, fitting into the hollow of the cylinder. Magnesium is readily volatile; so much so indeed, as to allow of its being purified by distillation in an atmosphere of hydrogen. It is a highly combustible metal, and burns brilliantly in air or oxygen, with a pure white flame. A magnesium wire, ignited at one end, will continue to burn through its entire length. Magnesium corresponds closely in its properties with zinc, and through zinc approximates to cadmium. The atomic weight of zinc is the mean of the atomic weights of magnesium

and cadmium; and the atomic volume of zinc is the mean of the atomic volumes of magnesium and cadmium.

Calcium.—This metal does not result from the action of sodium upon chloride of calcium, but is obtainable only by the electrolysis of that salt, rendered a conductor by fusion. It is a solid metal, of a somewhat yellowish colour. It is highly lustrous, but tarnishes quickly in the air, and gradually becomes converted throughout into oxide of calcium, or lime. It decomposes cold water rapidly with evolution of hydrogen. Its specific gravity is 1.58. It is moderately hard, malleable, and ductile. It has not been volatilized. When heated to redness in the air, it burns with a scintillating flash; but, in consequence of its want of volatility, does not inflame. Calcium bears to its congeners, strontium and barium, relations similar to those which magnesium bears to its congeners zinc and cadmium; save that the members of the calcic family are associated rather by an equality, and those of the magnesian family by a gradation of properties. The atomic volume and atomic weight of strontium are respectively the means of the atomic volumes and atomic weights of calcium and barium. Despite many points of resemblance, the differences between calcium and magnesium are well marked. Thus magnesium and zinc are volatile and inflammable metals, permanent in the air, scarcely acted upon by cold water, and obtainable by treating their respective chlorides with sodium. Calcium is neither volatile nor inflammable, is quickly oxidised in the air, is rapidly acted upon by water, and is not obtainable by treating its chloride with sodium. The hydrate of calcium is soluble in water, the hydrates of magnesium and of zinc insoluble. Hydrated chloride of calcium, when heated, evolves water, but the hydrated chlorides of magnesium and zinc evolve chlorhydric acid, &c.

Lithium is a very sparingly distributed element. It occurs native, in proportions varying from 3 to 12 per cent., in certain complex silicates, fluorides, and phosphates; and is obtained in the metallic form by the electrolysis of fused chloride of lithium. The specific gravity of lithium is 0.59. With the exception of bodies in the gaseous state, it is the lightest substance in nature. It floats upon every known liquid. One cubic inch of platinum balances $36\frac{1}{2}$ cubic inches of lithium. Lithium is a white-coloured lustrous metal, rapidly oxidised by exposure to the air. It is softer than lead, and may be cut with a knife, or squeezed between the fingers. It is readily obtained in the form of wire, by Matthiessen's process. It melts at 180°C , and at a higher temperature volatilizes. When heated to redness in the air, lithium takes fire, and burns with a brilliant, highly luminous, white flame, that is in curious contrast with the crimson colour which its compounds impart to the flame of ordinary combustibles. The presence of lithium in any substance is usually ascertained by means of this crimson-coloured flame, which, however, is altogether irreco gnizable in the presence of even a small quantity of sodium salts, owing to the intense yellow-coloured flame which they produce. Cartmell

has recently pointed out a ready mode of detecting the lithium coloration, even in the presence of a large excess of sodium salts, namely by viewing the flame through a layer of the blue solution of sulphate of indigo, which completely cuts off the yellow rays due to the sodium, but allows the uninterrupted transmission of the crimson rays due to the lithium. Gradational relations, similar to those of magnesium, zinc, and cadmium, exist between lithium, sodium, and potassium. In the solubility of its carbonate, in its degree of oxidisability, and in many other properties, sodium is strictly intermediate between its two congeners. De la Rue has observed, that metallic sodium from its inferior degree of oxidisability, may be preserved unacted upon in an aqueous solution of caustic soda. The atomic weight and atomic volume of sodium are respectively the means of the atomic weights and atomic volumes of lithium and potassium.

Lithium undoubtedly belongs to the same family as sodium and potassium. Like these two metals it is soft, readily fusible and volatile, highly oxidisable, and of lower specific gravity than water. Moreover, its hydrate and carbonate are sensibly soluble in water. But having regard to the totality of its characters in the free and combined states, and particularly to the properties of its hydrate, carbonate, and phosphate, it appears that the analogies of lithium to calcium and magnesium respectively, are scarcely less marked than are its relations to the true alkaline metals. It seems, indeed, as if the metals lithium, calcium, and magnesium stood upon the same level, and that while the gradation of lithium, sodium, and potassium, diverged in one direction, the gradation of magnesium, zinc, and cadmium diverged in another.

Lithium.	Calcium.	Magnesium.
Sodium.	Strontium.	Zincum.
Potassium.	Barium.	Cadmium.

Or, we might say, that, as regards their properties, potassium, barium, and cadmium, are highly specialized forms, while lithium, calcium, and magnesium are degraded or general forms, which, from the comparative absence of special characters, approximate to one another. The sums and means of the atomic volumes of the three groups of elements are shown below.

	Sums.	Means.
Mg. Zn. Cd. . . .	14.2	4.7
Ca. Sr. Ba. . . .	24.9	8.3
Li. Na. K. . . .	37.4	12.4
	<hr/>	<hr/>
	3) 76.5	3) 25.4
	<hr/>	<hr/>
	25.5	8.4

The atomic volume of the calcic family is observed to be exactly

intermediate between those of the lithian and magnesian families. Similar relations exist between the atomic weights of the three groups.

When, irrespective of these curious numerical relations between the different elements and groupings of elements, we find the groups characterized by distinctive but correlated properties, and the members of the groups associated by community of characters, and separated by gradational differences only, we perceive that the possession, by each element, of its own special properties, is not an accidental endowment, but is a necessary result of the development of one general comprehensive plan.

[W. O.]

Friday, March 18, 1859.

CHARLES WHEATSTONE, ESQ. F.R.S. Vice-President, in the Chair.

REV. WALTER MITCHELL, M.A.

On a New Method of rendering visible to the Eye some of the more abstruse problems of Crystallography, hitherto considered only as Mathematical Abstractions.

THE unpopularity of crystallography may be attributed to the difficulties so many people, especially those who have not had a good mathematical training, meet with in attempting to master the conception of forms involving some of the principles of solid geometry. To a certain extent this may be removed by a well-arranged system of solid models: thus the first three propositions of the 15th book of Euclid, "the inscription of a regular tetrahedron in a cube, of a regular octahedron in the tetrahedron, and of the octahedron in the cube," may be demonstrated to the eye by a dissected cube, illustrating the natural cleavage of fluor spar. Indeed the cleavage of a cube of fluor spar is a natural demonstration of the three principal propositions of the last book of Euclid's Elements of Geometry.

There are many propositions of crystallography which require some mechanical means beyond that of the use of solid models to make them appeal to the eye for clearer perception. The most perfectly symmetrical solid forms of the crystallographer belong to the cubical or tessular system. There are seven different kinds or orders of forms belonging to this system, perfectly symmetrical; four of which admit of an infinite variety of species. These forms are associated in nature as well as in their mathematical relations to each other. They are found in crystals of the same substance, either in their simple forms or else associated in combination with each other, in the different faces of a compound crystal; thus the cube, the octahedron, and the rhombic dodecahedron, are found as simple crystals of the diamond, or faces parallel to all three or two of them, may be discovered on a more complex natural crystal.

The three forms we have just enumerated, the cube, the regular octahedron, and the rhombic dodecahedron, may be considered as the permanent or limiting forms of the cubical system; they admit of no varieties; their angles, whether those of the inclination, of adjacent faces, or of the planes constituting their faces, are invariable; they are

also limiting forms. Between the octahedron and the rhombic dodecahedron we may conceive an infinite number of varieties of the three-faced octahedron, passing from the form of the octahedron to that of the rhombic dodecahedron ; similarly, the octahedron and the cube are limiting forms of an infinite series of twenty-four-faced trapezohedrons, and the cube and rhombic dodecahedron of a series of four-faced cubes. The forty-eight-faced scalenohedron or the six-faced octahedron is a form varying within the limits of all the others.

To represent to the eye the passage of all the varieties of these forms between their respective limits is the object of the mechanical contrivance which is the subject of this paper. A skeleton or armillary sphere is constructed of iron wire, so as to mark out the principal zones of the sphere of projection of the forms of the cubical system ; three circles are united at right angles to each other, so as to represent eight equilateral spherical triangles, each of whose sides are arcs of 90° . The six points where the arcs cross each other are the poles of the six faces of the cube ; the lines joining each pair of opposite poles represent the cubical axes, each axis being perpendicular to two faces of the cube which can be inscribed in the sphere. Each arc is now bisected. These twelve points of bisection are the poles of the rhombic dodecahedron ; the lines joining the opposite pairs of these poles are the rhombic axes, each of these axes being perpendicular to two faces of the rhombic dodecahedron inscribed in the spheres, or inscribed in the cube inscribed within the sphere. Let each of the eight equilateral spherical triangles be divided into six equal and similar spherical triangles by arcs, joining the angle of each triangle with the centre of its opposite side : the armillary portion of the sphere is now completed. The point within each of the eight equilateral spherical triangles, formed by the intersection of the three arcs by which it is divided, is the octahedral pole. There are of course eight of these ; the lines joining the opposite pairs of these poles are the octahedral axes, each one being perpendicular to two opposite faces of the regular octahedron inscribed in the sphere, or in the cube inscribed within the sphere. If we now join each pole of the octahedron with the three poles of the octahedron in the three adjacent equilateral spherical triangles by straight wires, and do this symmetrically for the eight poles, we shall then have the edges of the cube inscribed within our armillary sphere. The octahedral axes joining the opposite solid angles of this cube and the rhombic axes passing through the centres of each opposite edge.

Within this skeleton cube we now inscribe a regular octahedron, using elastic strings for its edges, by uniting the point where each cubical axis passes through the face of the cube, with the similar points on the two adjacent faces. Each face of the octahedron is therefore represented by an equilateral triangle of elastic cord. We now suppose each side of the eight equilateral triangles to be bisected. Every angle of the eight equilateral triangles is joined to the bisection of its opposite edge, by another series of elastic cords. We have now an octahedron inscribed, in the cube inscribed within our armillary sphere. Every face of the octahedron having marked upon it, the

traces formed by an imaginary plane passing through the zones of the sphere and its centre. It will now be seen that the cubical axes join the opposite solid angles of the octahedron; the rhombic axes, the bisections of its opposite edges; while the octahedral axes pass through the intersections of the elastic cords, which join each solid angle of the octahedron with the centres of the edges opposite to it.

The points where the elastic cords meet, and the octahedral axes pass through the faces of the octahedron are now fastened to cords. These cords are made to run round pulleys and are united together, so that by pulling them simultaneously, the points uniting, every one of the three elastic cords which are described on the face of the inscribed octahedron can be made to travel uniformly and symmetrically along each of the octahedral axes from the face of the octahedron to the solid angle of the circumscribing cube. Another series of cords are united to each of the four elastic cords, which meet at the point bisecting each of the edges of the inscribed octahedron. These, by a similar contrivance, are made to draw these points along the rhombic axes. The instrument is now completed. By simply pulling the eight cords united together, which cause the elastic cords to ascend the octahedral axes, the inscribed octahedron passes through every form of the three-faced octahedron till it reaches the limiting form of the rhombic dodecahedron. Each three-faced octahedron being inscribed within the cube, inscribed within the sphere.

In a similar manner, by pulling the cords, running along the rhombic axes in combination with those running along the octahedral axes, all the other forms are shown as passing within their prescribed limits. As soon as the cords are loosened, the elastic bands immediately resume the form of the inscribed octahedron. In addition to these forms, the instrument also can be made to demonstrate the passage of all the hemihedral forms of the cubical system with inclined faces within their limits. In this manner it was demonstrated that this instrument can make visible to the eye all the changes and varieties of an interesting series of forms and their mutual relations, which could otherwise only be conceived by a considerable power of mathematical abstraction. This armillary sphere, by some other small additions, can be made use of for tracing out some of the most beautiful portions of the zone-theory of the poles of crystals.

[W. M.]

Friday, April 1, 1859.

SIR RODERICK I. MURCHISON, D.C.L. F.R.S. Vice-President,
in the Chair.

NEVIL STORY MASKELYNE, Esq. M.A.

*On the Insight hitherto obtained into the Nature of the Crystal Molecule by the instrumentality of Light.**

THE horizon of man's view extends in two directions. The one is turned towards the infinitely vast, and carries his eye into the regions of space, spanning distances that leave his world a speck in creation. The other direction along which he strains his gaze, is into the infinitesimal; and the microcosm of the crystal is the region it must traverse before it can reach the ultimate units of material consistence, which form the centres of chemical force. That region, however, is to be explored by the reason rather than by the eye, for the minutest thing cognisable by the microscope is left far behind in the first step taken downwards into the crystal world.

In a crystal there exists a complete *regularity of arrangement* and of the distribution of its powers of resisting, of transmitting, or of converting and modifying any forces that may solicit it. Of this, to a great extent, the external symmetry of its facettes is an expression. But the physical properties of the crystal afford the most perfect evidence of it.

The different varieties of symmetry, exhibited by outward crystal-line shape, are found to indicate, with much precision, a correspondingly

* In this notice a larger space is given to preliminary and other details than could be devoted to them in an hour's discourse. This was felt to be necessary, partly because the speaker entered more on these points than he had intended in the scheme of his discourse; and having done so it is his wish to give a clear statement of them: partly too because the subsequent views for which he is much indebted to the conversations and private letters of his friend Professor Grailich, are more intelligible to a general audience by the help of such preliminary explanations. He believes they may render a beautiful subject—which must be difficult, and cannot be popular in the childish sense of the word—sufficiently intelligible, by patient study, to be interesting to persons such as those who did him the honour of forming his audience on the 1st of April.

ordered arrangement in the physical properties of the various crystals that illustrate them.

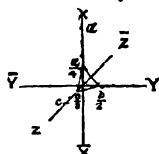
The definition of the terms *crystallographic axes*, *parameters*, *indices*, and *morphological axes* being necessary for elucidating what was to follow, were given. Thus *crystallographic axes* are geometrical directions, determined by the intersections of any three planes of the crystal. They are taken as axes of coordinates, and called severally x y and z , and such are chosen as express in the most simple and smallest numbers the relations of all the planes of the crystal to one another. These relations are indicated by the *Law of Crystallography*, viz., by this, that a plane parallel to any facette will cut these axes at certain definite relative distances, measured from the centre at which the axes cross, and controlled by the principle to be next enunciated. Certain of such planes can always be found in every crystal species for which the ratios of these distances are simpler than in any others. The distances for such particular planes are called the *parameters* of that species, and their values are generally indicated by the letters a b and c , which represent the distances measured severally along the axes x y and z . The ratios of these parameters cannot be expressed (except, of course, where they are equal) by any rational numbers. In this respect they bear some analogy to the chemical equivalents; thus in topaz the parameters are

$$a : b : c :: 1 : 0.5284 : 0.47698 \text{ approximately.}$$

Now every facette on any topaz crystal must cut the axes at distances, the ratios of which are represented by very simple fractions of these numbers, such as

$$\frac{1}{2} \frac{1}{3} \frac{1}{4} \text{ \&c.}$$

Hence any facette represented by the form (423) will be parallel to a plane cutting the axes x y z in points whose distances along these axes are found by the proportions $\frac{1}{4} a$ along axis x : $\frac{1}{2} b$ along axis y : $\frac{1}{3} c$ along the axis z . Just as by the law of definite proportions in chemistry, if the equivalent of iron be taken as 28.042 and of oxygen 8.



We have 28.042 ... iron + 8 oxygen, forming one oxide of iron,
 2×28.042 ... iron + 3×8 oxygen forming another do.

4, 2, 3, are the *indices* of that facette, and by the symbol (423) is indicated a group of eight facettes which the law of symmetry of the system requires, and which will, therefore, be found on every complete topaz crystal that carries one of these facettes of that form. The indices are generally indicated by the letters h k l , which, therefore, express rational numbers, while the parameters a b c always (where unequal) express irrational numbers.

The term *morphological axis* was defined as an axis round which the facettes are symmetrically arranged, but which is not necessarily a crystallographic axis (e.g., in the rhombohedron).

The terms *elasticity*, and *axes of elasticity*, were next explained—the former term as implying a power of counter-resistance to any force tending to displace the particles of the crystal (e.g., the compressing force of a blow, or any vibration, such as sound, &c.), the axes of elasticity being those directions in the crystal along which alone the displacement and the counter resistance opposed to it by the crystal, coincide and operate in the same line. A force acting in any other direction is met by counter-forces distributed along the directions of these axes of elasticity.

These definitions being explained, the speaker entered on a short illustration of the laws of crystallographic symmetry in crystals, exhibiting, by means of diagrams, their general morphological relations, so far as was necessary for the subsequent discussion, and pointed out the analogy in complex crystal forms, with the symmetry of certain floral types of form.

The representation of the magnitude and directions of the axes of elasticity in each crystalline system was shown to be possible by means of one of three solid figures.

These, in the case of the octahedral system—from the mutual convertibility

of every axis of elasticity in it—were shown to be represented by a sphere. No more was said of this system.

The pyramidal and rhombohedral systems were morphologically associated, by the fact of each having one morphological axis, round which a perfect symmetry reigned—though crystallographic laws ranged the facettes in multiples of 4 round the axis of the first, and of 3 round that of the second. The elasticity in these crystals was represented by a spheroid—prolate (called negative), or oblate (called positive), according as the elasticity was greater in an axial or an equatorial direction.

The prismatic system was represented, as regarded its elasticity, by an ellipsoid, a figure whose axes are all unequal, though rectangular.

The oblique systems, also approximately represented by ellipsoids, were discussed later.

These preliminary statements having been gone through, the speaker entered on the question of the internal molecular arrangement of the crystal, and first briefly reviewed what has been established regarding the physical relations of crystals.

He introduced the name of Professor Grailich, of Vienna, in connection with this subject as one who, with his pupils, had worked over the whole of this large field of research, and had both added much to the facts themselves, and contributed greatly towards the extension of the theoretical views that must be called in to explain them. Some of the more recent of these results of Professor Grailich and his *collaborateur*, Dr. Viktor von Lang, formed the chief subject matter of what followed.

The experiments made hitherto to determine the action of a crystal upon different physical powers, were then reviewed and shown to lead to the general conclusion that there is a very close relation between its morphological (and therefore geometrical) symmetry and its physical properties.

The mechanical elasticity possessed by the crystal in its different directions, may be examined through its cohesion, as indicated by its cleavage, its hardness, its acoustic properties, &c

THE CLEAVAGE, where it exists in a crystal, invariably occurs in the direction of actual or of (in obedience to the great crystallographic law), possible crystal-planes, and is furthermore found to follow planes represented by very simple indices. Besides this relation between the direction in which the crystal splits and its crystallographic form, there is also exhibited a remarkable relation between the degrees of facility in which the substance yields to the cleaving force and the symmetry of the crystal. Such crystals as have equal parameters exhibiting equal facility of cleavage in three directions; such as have two only exhibit equally easy cleavage in two directions.

THE HARDNESS of a crystal also varies on its different facettes, and in different directions on the same facette; and it would seem to be a general law that the greatest hardness is exhibited in a direction and on a plane at right angles to that of cleavage, and that the hardness graduates in its degree in proportion as the plane experimented on tends to coincide with that plane. [Frankenheim, Franz, Grailich and Pekarek.]

THE ACOUSTIC characters of a crystal are of an analogous kind. So far as they have been investigated they seem to follow the law of its

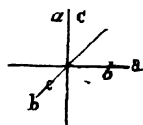
symmetry. Thus rhombohedral crystals exhibit a triple acoustic symmetry round the morphological axis; and the axes of acoustic elasticity, appear to coincide with the crystallographic axes, *i.e.*, with the intersections of the planes of the fundamental rhombohedron. In some cases the the acoustic characters of the crystal (indicated by the tones produced, as also by the lines into which finely sprinkled particles arrange themselves on a plate of the substance when set vibrating by a violin bow), seem to provide a more subtle means of investigating its elastic structure than the coarser methods of cleavage and hardness [Savart's Experiments—this is especially exemplified in the case of quartz], and crystal acoustics would, in this respect, seem to offer a fertile field for investigation.

IN THE MAGNETIC relations of the several parts of a crystal much has been done [Faraday, Plucker, Tyndall and Knoblauch, Grailich and Von Lang]. The results of Professors Tyndall and Knoblauch have been completely confirmed and extended by Grailich and Von Lang, who have added to their results a large series of magne-crystalline determinations, and expressed them by a nomenclature at once comprehensive and concise. Tyndall and Knoblauch have shown that the *intensity* of the dia-magnetic (*i.e.*, equatorially tending), or of the paramagnetic (*i.e.*, axially tending) action, depends on the density of the substance along particular directions. The Vienna observers prove that isomorphous bodies, of similar (dia- or para-) magnetic kinds, have a similar magnetic set; and that those of opposite kinds (one of para- the other of dia-), magnetic nature, comport themselves in ways precisely inverse to one another, and that thus the magnetic *orientation** depends upon the different relative densities of the crystal along different crystallographic directions in it.

There is evidently here a qualitative action (an axial or an equatorial set) which is to be distinguished from the amount of energy, of this setting or directive force, in various directions of the crystal. The former depends on the chemical nature of the substance; the latter solely on the crystalline arrangement.

* By orientation is implied the directions in space, relatively to some given directions, of any axes (whether crystallographic, magnetic, acoustic, optic, thermic, or any other). Thus if *a b c* indicate the order of magnitude of the parameters of a trimetric crystal, say one of aragonite, and these are fixed in position, *a*, for instance, being vertical, *b* horizontal, running from right to left, and *c* horizontal, running from front to back, the magnetic orientation of such a crystal is found to be such that the line *c* has the strongest tendency to stand equatorially. Aragonite is a diamagnetic substance, and this, together with its orientation, is designated by Professor Grailich by the symbol δ (*c a b*) which serves to indicate that the lines of greatest, mean, and least magnetic action are in the order

c a b, the greatest crystallographic axis being that of least magnetic action (and therefore in this diamagnetic substance also that of greatest molecular density), while the mean and least crystallographic axes are respectively the least and the mean of the magne-crystalline axes. The magnetic orientation of the mineral staurolite, which is a para-magnetic substance, is expressed by π (*a c b*), so that here the same axis is the direction at once of the greatest crystallographic parameter and of the greatest magnetic action, *i.e.*, the axis of greatest density,



The passage of ELECTRICITY through crystals has been studied [Wiedemann, Senarmont, Knoblauch], and given general results indicative of analogy with those obtained in respect to the transmission of light and heat; while the remarkable development by heating or cooling a crystal, of electric tensions on parts of it morphologically polar to each other [analogue and antilogous poles in terminally-polar, and also central-polar pyro-electricity—Riess and Rose, Karsten Pasteur, &c.] indicates that here, as in magnetically polar substances, and as in the alterations in volume effected by change of temperature, the powers that, so to say, reside in the crystal, and of which its crystalline form is the outward expression, are potent to modify the character and the amount of the tensions induced by the natural forces, which we call electricity, magnetism, and heat, and that those powers have thereby a direction or localisation imposed on them in accordance with the crystalline structure.

HEAT, indeed, must be viewed in more than one *mode operandi*. As *radiant heat*; in the influence of the crystal upon its forward propagation; in its polarisation, absorption, or transmission by (*i.e.* the diathermanence of) the crystal, we have to associate it intimately with light, to view it in short, as light endowed with longer wave length.

As *heat of temperature* (intensity of thermic excitement) we must follow up its action on crystals as an agent causing increase of volume, and therewith inducing a series of concomitant results of the highest interest. In this respect it affords one of the most instructive means at our disposal for the examination of crystal structure. Upon this Professor Grailich and Dr. Viktor Von Lang have brought mathematical analysis to bear; and by showing that the increase of temperature, while changing the relative values of the parameters of a crystal, never alters the irrational character of those parameters, have given to Mitscherlich's beautiful and well-known results a new significance, as Von Lang has also done with Rudberg's investigation on the change in the action of aragonite on light induced by a change of temperature. They have shown that all which is symbolised by the indices (the general symbol $(h\ k\ l)$, in a crystal—its symmetry, and therefore its system—remains unaltered: the lengths of the parameters may vary, the inclination of a leaning axis in the oblique systems, may change by change of temperature, but the principle which Grailich establishes as the "Law of Conservation of Zones," remains presiding over the general crystal form, so that it is impossible for a crystal, by the mere agency of a changing temperature, to drift from one system into another.*

while the mean and least parameters are the directions respectively of the least and mean magne-crystalline set.

* *Thermic axes* are those directions in a crystal along which it is altered, by change of temperature, *only in linear dimensions*. They are are fixed crystallographic directions. Every other crystallographic line that can be drawn through the crystal, changes not in length only, but also in *direction* relatively to these axes.

But it is to **LIGHT** that we are to look as the most subtle instrument for aiding our reason in scrutinizing the inner nature, or most intimate structure of the molecular system, which we call a crystal. It seems a true prerogative of light to do this. The frequent transparency, and the varied colours in different directions of so many crystals; and the changes which the light is subject to within the crystal, its polarisation, absorption, fluorescence, and the other modifications it undergoes, all point to this most subtle agency as a discriminative power the best adapted for our purpose.

In any of the systems under consideration, the light, on entering a crystal, is, except along certain directions, divided and polarised. The two polarised rays into which it becomes thus divided pursue new and different paths in the crystal, each ray differing from the other in the velocity of its propagation, the crystal retarding the progress of the ray vibrating in one plane, more than it does the ray vibrating in a plane polar to this. The spheroids, or ellipsoids of elasticity before alluded to present admirable geometrical expressions for the degree and relative amounts of the retardations effected, and for the directions of the planes of vibration thus induced by the crystal on the waves of light. Without entering into an explanation of the polarisation of light, or its precise relations to the directions of optical elasticity in the crystal, it was deemed enough to remark that parallel to one direction the sections of the spheroid are circles, and that the (locus of, or) line formed by the consecutive centres of these circles is the morphological as well as the optical axis of all crystals belonging to those systems, whose elasticity can be represented in magnitude and direction by a spheroid (the pyramidal—rhombohedral). In the prismatic, (and approximately in the clinohedric systems,) whose elasticity is represented by an ellipsoid, there are two circular sections that may be made through the centre of the ellipsoid of elasticity.

In either case lines perpendicular to these circles are the "*optic axes*" of these systems, that is to say, are directions along which a ray goes with only one velocity, and is, therefore, not broken up and polarized; as can be demonstrated by a very simple geometrical construction. The spheroidally elastic systems have, therefore, one optic axis—one only direction along which light passes unchanged (*uniaxial systems*). The systems whose optical elasticity is represented by ellipsoids have two such directions (*biaxial systems*). Moreover, in the latter the plane of these can be readily shown to lie necessarily in the same plane as contains the greatest and least axis of the ellipsoid, *i.e.* the greatest and least axis of elasticity.

The *first mean line* of the optic axes in biaxial crystals, is the line bisecting the *acute* angle formed by the optic axes. In prismatic crystals it is, according as the mean elastic axis is proportionately small or large, either the greatest axis of optical elasticity (optically negative crystal), or the least (an optically positive crystal). The *second mean line* is the axis bisecting the *obtuse* angle, formed by the optic axes. The *law of the prismatic system* regarding the position of the optic axes for different colours, appears to be that the first mean

line of the optic axes is the same for every colour, in any given crystal species, but that the angle of the optic axes for each colour may be different. Furthermore, in this system, the axes of optical elasticity, and the *mean lines*, as well as the planes containing the optic axes, coincide in *direction* with the morphological axes.

A beam of polarized light was employed to throw on a screen the stauroscopic phenomena produced in sections of crystals, cut perpendicularly to their optic axes, or to the first mean lines of these; and, by the use of absorbing coloured glasses, the different optic axes were shown to diverge more for some colours than for others in the prismatic system.

Thus Rochelle salt exhibits the centres of the rings or brushes as far more divergent for the red rays than for blue, while in aragonite the converse is the case, though not in so eminent a degree. Mellate of ammonia (and Brookite also) exhibit (as shown by Grailich) the wonderful fact of a divergence of the red rays with the optic axes in the plane xz , while the optic axes for the blue rays lie in the plane yz , and those for green, converge at the centre, into a uniaxial system.

The more complicated phenomena exhibited by sections of crystals belonging to the clinohedric (*oblique and anorthic*) systems were next made the subject of illustration, partly by diagrams and in part too by experiment. In the monoclinohedric (the *singly leaning prismatic system*), there is one *plane* of morphological symmetry, and it contains the two crystallographic axes that are inclined to each other; the third is called "the axis of symmetry," and is the only true crystallographic axis in this system, that is fixed by morphological conditions. The laws of the distribution *and of the dispersion* of the optic axes in this system, as given by Angstrom [Beer's *Höhere Optik*, and Grailich's Translation of Professor Miller's *Crystallography*], are—1st, That this morphological axis is also one *axis of polarisation** for all colours, without dispersion (*i.e.* without any divergence of the directions of vibration of the rays for different colours); but that the other two *axes of polarisation* are *rectangular* axes, and are dispersed for different colours differently in the plane of symmetry; and 2ndly [Angstrom], there are three different cases peculiar to this system possible: gypsum illustrates one of these, that namely, in which the morphological axis is the *second* mean line, and the axes of the blue and red rays stand to each other in the positions of B R B R.

Borax illustrated another of these cases, where the planes of the optic axes for the blue and for the red were so dispersed in the plane of symmetry as to be crossed thus: $\begin{matrix} R & B \\ B & R \end{matrix}$ the morphological axis being the *first mean line*.

Finally, Adularia illustrates the position of the optic axes, in which

* The use of the word *axis of polarisation* instead of *axis of elasticity*, in this case is afterwards explained.

the planes containing them, for each colour, cross the plane of symmetry, and the morphological axis is the *second* mean line, the first mean *lines* being dispersed for each colour along the plane of symmetry.

This position is that of $\begin{matrix} R & R. \\ B & B. \end{matrix}$

Thus far the conformity between crystalline symmetry and the distribution of optical elasticity, would *seem* sufficiently near to bring the latter into the same category with the elasticity exhibited by the crystals under the solicitation of less subtle forces. But a nearer view of the phenomena goes far to dispel this expectation.

For all the results that had been previously reviewed, an explanation more or less complete may be found in a simple hypothesis; namely this, that the centres of gravity of the crystal-molecules are always arranged in planes which represent either actual or possible facettes: while the relative distances of molecule from molecule are the same in the same direction, but different for different directions. To disturb a molecule so as to move it out of its plane, would be to destroy the integrity of the molecular system. Magnetic orientation would only indicate the lines of greater or less distance between the molecules; increase of thermic intensity (temperature), too, only causes a greater interval between the molecules without altering the crystallographic relations of the planes they lie in—albeit that this increase of interval is different in different directions. These, and, in all probability also, the other as yet less elaborated results of physical experiment, are thus explicable as dependent on the disposition of the mass centres of the molecules, and are so capable of being rendered subordinate to the fundamental law of crystallography.

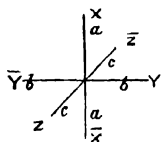
But the hypothesis above sketched in outline fails to explain a series of facts which Light reveals to us, and which show that the optical properties of a crystal cannot be directly dependent on the arrangement of the molecules. Among these are the following:—

Firstly. The action of increased temperature on a crystal, while it alters the volume of the crystal, does so by changing the values of the parameters, changing thereby too the refractive powers of the substance. As such refractive powers vary generally inversely as the amount of elasticity and directly as the density of the light-transmitting medium varies, we might expect the variations in the parameters and refractive indices and also in the axes of elasticity to follow *pari passu* with the changes in the molecular density in different directions as the crystal becomes expanded: nothing of the kind occurs. The axes of optical elasticity vary indeed (in aragonite for instance), but their orientation is quite dissimilar from that of the altered crystallographic axes; *i. e.*, the directions of the greatest mean and least parameters of the heated crystal, which measure its physical density, entirely differ from the corresponding directions of the greatest mean and least of the new axes of optical elasticity.

2ndly. The speaker had already alluded to the remarkable fact of the crossing of the planes of the optic axes for different colours in mellate

of ammonia, brookite [Grailich], and other crystals, such as at a high temperature, glauberite [Brewster, Descloiseaux], and gypsum [Mitscherlich]. It is strange to think that thus in one and the same substance there should co-exist a principal direction of vibration of a different order (*e.g.* the *a* axis for the red, with the *b* axis for the violet), for different colours in one and the same direction.

3rdly. Again, the prolific labours of Grailich, and his pupils, have established a most interesting and important law regarding isomorphous substances, viz. this: that the optical similarity between any series of isomorphous bodies frequently diminishes in proportion as the isomorphous chemical substances, *not* common to the different bodies, preponderate over those which are common to them.



The *crystallographic* orientation of the form common to the isomorphous group of the following substances, is expressed by the parameters in the order of their magnitude, *a b c*.

In sulphate of potash the *optical elasticity* is expressed by the symbol $\begin{smallmatrix} a & c & b \\ + & & + \end{smallmatrix}$; in sulphate of ammonia by $\begin{smallmatrix} b & a & c \\ + & & + \end{smallmatrix}$; in chromate of potash, by $\begin{smallmatrix} a & c & b \\ + & & + \end{smallmatrix}$; the order of the letters indicating in each case the orientation

of the different magnitudes, as expressed by the letter (*a* being greater than *b* and *b* than *c*), and as compared with the directions in space of the original crystallographic parameters *a b c*.

Thus the chemical *substance* entirely overrules the crystalline arrangement in impressing on the crystallized body an optical elasticity; so that we are driven to seek the *fundamental cause of optical character, not in the arrangement of molecules, but in the inner nature, constitution, structure of these*.

How, then, are these results, discordant as they are with crystallographic facts, to be reconciled with that general harmony between morphological symmetry and crystallo-optical phenomena. which we have already in part accepted?

To this question, the answer is not readily found. It is, however, the more probable view, that the chemical units of matter (of the nature of which we are profoundly ignorant, and are without even analogies to guide us; but which the atomic theorists, assume to be compounded of atoms in some form of geometrical arrangement, and clustered together in chemical groups), form the basis of the crystalline system: that these are ordinated (with the ether of the mathematical theory) in higher groups which form the ultimate crystal-units-of-mass, or crystal molecules—but that the form and internal arrangement of the molecule bears a close analogy, and a general identity of symmetry, with the form of the crystal of which it is the constituent mass-unit.

The chemical units of mass we can at any rate treat as distinct mechanical units with centres of gravity, and centres of volume (probably distinct), and endowed with powers that cannot be the same in different directions. These then would thus upbuild the molecular

units of mass of the crystal, and would be arranged in the interior of those molecules in a manner closely *analogous* to the arrangement of the molecules in the crystal itself; which, as was before stated, may be looked on (at any rate as an illustration to the mind of crystallo-physical facts) as so co-ordinated in direction as that every plane in which a series of the centres of gravity of the molecules lies, must be (an actual or a possible) crystal plane. The mysterious ether assumed and adopted by the undulatory theory plays its part within the molecules and enshrouds its constituent chemical units of mass.

On the one hand, then, axes, such as those of acoustic elasticity, of magnetic induction, or thermic axes, belong to *the crystal as a whole*, depending for their magnitude and direction on the static condition and the relative arrangement of the molecules *inter se*. The axes of optical elasticity on the other hand will be the axes of elasticity of the *molecule* itself, that is, of the ether within it. But the elasticity of the ether, as exhibited by the velocity with which a ray is propagated in it along its different directions is controlled by the action of the reposing ponderable units (or atoms at rest) on the ether particles in motion. As long as the directions in which the ether is constrained to vibrate, by reason of this perturbing action—its axes of elasticity—are continuous throughout the crystal (as will be the case if the neighbouring molecules are arranged *inter se* in the same symmetry as the ether particles in any one of the molecules, so that the neighbouring molecules do not distract those directions by a perturbing action oblique to theirs,) so long will the influence of the material particles be confined to the exercise of a constraint on the *velocity* of each wave, whatever its length. *i.e.* colour; but it will not cause any dispersion of these. This condition of coincidence in direction of the crystalline and molecular axes occurs only in the rectangular systems; and in the biaxial ones, therefore, only in the Prismatic system. But the coincidence is confined to *direction*, neither the magnitude of the axes, nor their orientation (as had been shown) presenting any identity.

In the oblique systems there is no reason to assume the obliquity in the molecule as of the same inclination as that of the inclined axes of the crystal. Thus these two sets of oblique axes cannot coincide in direction. Hence the perturbing influence of the ponderable matter operates, by reason of this want of coincidence, in directions other than those of the axes of the individual molecule, and extends its results to the *directions of vibration*, that is to say, effects the *dispersion* of these directions for different colours. In fact, for any light-wave now to traverse the crystal without being broken up, it must take directions neither coincident with the axes of elasticity of the molecule, nor with the crystalline axes. These directions will have different orientation for different colours), and, not being coincident with the thermic axes, will vary with the temperature. They will be rectangular axes—axes of a polarisation-ellipsoid, different for each colour—and will indicate the directions of the principal planes of polarisation, which must be normal to each other, and will also prescribe the limits of the

wave surface. They are the resultant *axes of polarisation* before alluded to; they have no statical significance in the crystal.

But this subject enters here into the domain of mathematical analysis. Indeed, though experiment has effected much, and mathematical interpretation more, for the establishment of the laws that control the difficult phenomena exhibited by oblique crystals, the subject is still one in which much has to be done, especially in determining the true axes of elasticity in these crystals.

The last subject touched on, and that briefly, was the impossibility of explaining the growth of a crystal by any architectural view, like that of Haüy, *i.e.* by any view that supposes the crystal developed by the addition, one by one, as it were, of molecules, endowed only with forces acting at minute distances and depositing themselves by virtue of these alone in their position. The facts connected with the growth of a crystal in its mother-liquor point to quite a different conclusion.

The simultaneous production of the corresponding facettes, however minute they may be, and however complicated the crystal form, upon the opposite ends or parts of it, and in the precise positions where symmetry requires them, needs for its explanation something akin to an insinua in the molecules, if we are to suppose them so to deposit themselves, as that their deposition is independent of influences extending at once over the whole crystal, and to the mother liquid investing it.

Hemihedrism points to the same result.*

So too how are the infinite numbers of tessellated crystalline fragments that interpenetrate without any symmetrical orientation so many crystals [Leydolt's Etching Experiments on Quartz, Apophyllite, &c.] without interfering with their general form, to be explained?

Finally, the forms of crystals, say of fluor spar, or of calc spar, from the same mine, are similar, while those met with in a neighbouring locality, where the conditions of deposit were different, are different from these. The typical forms of barytes as found in Cumberland, or in Auvergne, Schemnitz, &c. are different for each locality, and so are often the hornblendes that occur in different rocks. Over large extents of country a mineral (*e.g.* the augite of Southern Tyrol) will present constantly the same combinations of crystalline form. Salt is deposited in cubes from its simple aqueous solution,—in octahedra when that solution contains uric acid; and alum presents the form of the cube when alumina is present in excess.

In all these cases, therefore,—indeed, in every case—the growth of a crystal is an inexplicable thing, so long as we endeavour to trace its cause to powers residing in and confined to the molecules. A crystal, like a plant, is developed in a medium, and as the plant owes the special peculiarities of its individual form, notwithstanding the seemingly perfect freedom of its growth, to special circumstances in the

* Grailich connects hemihedrism (*i.e.* the development of only the alternate planes required by the symmetry of the system of a crystal) with a difference in position of the centres of gravity and centres of volume in the molecules.—(*Private Communication.*)

soil, the air, the weather during that growth ; and its general similarity to other plants of its kind, to the organic laws that controul the conditions of its species ; so must the crystal be considered as the result of many co-operating influences, including those of the foreign constituents of the mother liquid, those of temperature and other physical conditions, and involving the principle that the molecules, whether those deposited, or those about to become so, affect and are affected by—and that to considerable distances—the whole of the formed and forming crystal matter.

It would be as useless to expect to explain the growth of a crystal without some such view as this, as to endeavour to account for the growth or outward form of a particular plant by the development of a single leaf.

In closing the remarks made in this discourse upon the theoretical bearings of crystallo-physical and especially optical investigation, on our views of the structure and constitution of crystals, the speaker could only allude to the important *practical* services they have already rendered to mineralogy, especially in the able hands of M. Descloiseaux in Paris, who has been enabled to determine several mineralogical species by their means.

[N. S.-M.]

Friday, June 10, 1859.

THE PRINCE CONSORT, Vice-Patron, in the Chair.

JOHN TYNDALL, Esq. F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION.

On the Transmission of Heat of different qualities through Gases of different kinds.

SOME analogies between sound and light were first pointed out: a spectrum from the electric light was thrown upon a screen—the spectrum was to the eye what an orchestra was to the ear—the different colours were analogous to notes of different pitch. But beyond the visible spectrum in both directions there were rays which excited no impression of light. Those at the red end excited heat, and the reason why they failed to excite light probably was that they never reached the retina at all. This followed from the experiments of Brücke and Knoblauch. These obscure rays had been discovered by Sir Wm. Herschel, and the speaker demonstrated their existence by placing a thermo-electric pile near to the red end of the spectrum, but still outside of it. The needle of a large galvanometer connected with the pile was deflected and came to rest in a position about 45 degrees from zero. A glass cell, containing the transparent vitreous humour of the eye of an ox, was now placed in the path of the rays: the *light* of the spectrum was not perceptibly diminished, but the needle of the galvanometer fell to zero, thus proving that the obscure rays of the spectrum, to which the galvanometric deflection was due, were wholly absorbed by the humours of the eye.

Reference was made to the excellent researches of Melloni. In a simple and ingenious manner he had proved the law of inverse squares to be true of radiant heat passing through air, and the eminent Italian inferred from his experiments that for a distance of 18 or 20 feet, the action of air upon radiant heat was totally inappreciable. This is the only experimental result now known regarding the transmission of radiant heat from terrestrial sources through air; with regard to its transmission through other gases it was believed that we were without any information.

It was, however, very desirable to examine the action of such media—desirable on purely scientific grounds, and also on account of certain speculations which had been based upon the supposed deportment of the atmosphere as regards radiant heat. These speculations were originated by Fourier; but it was to M. Pouillet's celebrated Memoir, and the recent excellent paper of Mr. Hopkins, to which we were

indebted for their chief development. It was supposed that the rays from the sun and fixed stars could reach the earth through the atmosphere more easily than the rays emanating from the earth could get back into space. This view required experimental verification, and the more so, as the only experiment we possessed was the negative one of Melloni, to which reference has been already made.

The energetic action of the solid and liquid compounds into which the element hydrogen enters, suggested the thought that hydrogen gas might act more powerfully than air, and the following means were devised to test this idea. A tube was constructed, having its ends stopped air-tight by polished plates of rock-salt held between suitable washers, which salt is known to be transparent to heat of all kinds; the tube could be attached to an air-pump and exhausted, and any required gas or vapour could be admitted into it. A thermo-electric pile being placed at one end of the tube, and a source of heat at the other, the needle of an extremely sensitive galvanometer connected with the pile was deflected. After it had come to rest, the air was pumped from the tube, and the needle was carefully observed to see whether the removal of the air had any influence on the transmission of the heat. No such influence showed itself—the needle remained perfectly steady. A similar result was obtained when hydrogen gas was used instead of air.

Thus foiled, the speaker put his questions to Nature in the following way: a source of heat, having a temperature of about $300^{\circ}\text{C}.$ was placed at one end of the tube, and a thermo-electric pile at the other—a large deflection was the consequence. Round the astatic needle, however, a second wire was coiled, thus forming a so-called differential galvanometer; a second pile was connected with this second wire, so that the current from it circulated round the needle in a direction opposed to that of the current from the first pile. The second pile was caused to approach the source of heat until both currents exactly neutralised each other, and the needle stood at zero. Here then we had two powerful forces in equilibrium, and the question now was whether the removal of the air from the tube would disturb this balance. A few strokes of the air-pump decided the question, and on the entire removal of the air the current from the pile at the end of the tube predominated over its antagonist from 40° to 50° . On readmitting the air the needle again fell to zero; thus proving beyond a doubt that the air within the tube intercepted a portion of the radiant heat.

The same method was applied with other gases, and with most remarkable results. Gases differ probably as much among themselves with regard to their action upon radiant heat as liquids and solids do. Some gases bear the same relation to others that alum does to rock-salt. The speaker compared the action of perfectly transparent coal-gas with perfectly transparent atmospheric air. To render the effect visible to the audience, a large plano-convex lens was fixed between two upright stands at a certain height above a delicate galvanometer. The dial of the instrument was illuminated by a sheaf of rays from an

electric lamp, the sheaf being sent through a solution of alum to sift it of its heat, and thus avoid the formation of air-currents within the glass shade of the instrument. Above the lens was placed a looking-glass, so inclined that the magnified image of the dial was thrown upon a screen, where the movements of the needle could be distinctly observed by the whole audience. Air was first examined, the currents from the two piles being equilibrated in the manner described, the tube was exhausted, and a small but perfectly sensible deflection was the result. It was next arranged that the current from the pile at the end of the tube predominated greatly over its antagonist. Dry coal-gas was now admitted into the tube, and its action upon the radiant heat was so energetic, the quantity of heat which it cut off was so great, that the needle of the galvanometer was seen to move from about 80° on one side of zero to 80° on the other. On exhausting the tube the radiant heat passed copiously through it, and the needle returned to its first position.

Similar differences have also been established in the case of vapours. As representatives of this diverse action, the vapour of ether and of bisulphide of carbon may be taken. For equal volumes, the quantity of heat intercepted by the former is enormously greater than that intercepted by the latter.

To test the influence of *quality*, the following experiment was devised. A powerful lime light was placed at one end of the tube, and the rays from it, concentrated by a convex lens, were sent through the tube, having previously been caused to pass through a thin layer of pure water. The heat of the luminous beam excited a thermo-electric current in the pile at the end of the exhausted tube; and this current being neutralised by the current from the second pile, coal-gas was admitted. This powerful gas, however, had no sensible effect upon the heat selected from the lime light; while the same quantity of heat, from an obscure source*, was strongly affected.

The bearing of this experiment upon the action of planetary atmospheres is obvious. The solar heat possesses, in a far higher degree than that of the lime light, the power of crossing an atmosphere; but, and when the heat is absorbed by the planet, it is so changed in quality that the rays emanating from the planet cannot get with the same freedom back into space. Thus the atmosphere admits of the entrance of the solar heat, but checks its exit; and the result is a tendency to accumulate heat at the surface of the planet.

In the admirable paper of M. Pouillet already referred to, this action is regarded as the cause of the lower atmospheric strata being warmer than the higher ones; and Mr. Hopkins has shown the possible influence of such atmospheres upon the life of a planet situated at a great distance from the sun. We have hitherto confined our

* The quantity of heat is measured by the amount of the galvanometric deflection which it produces; its power of passing through media may be taken as a test of *quality*.

attention to solar heat; but were the sun abolished, and did stellar heat alone remain, it is possible that an atmosphere which permits advance, and cuts off retreat, might eventually cause such an accumulation of small savings as to render a planet withdrawn entirely from the influence of the sun a warm dwelling-place. But whatever be the fate of the speculation, the experimental fact abides—that gases absorb radiant heat of different qualities in different degrees; and the action of the atmosphere is merely a particular case of the inquiry in which the speaker was at present engaged. *

[J. T.]

* While correcting the proof of this abstract, I learned that Dr. Franz had arrived at the conclusion that an absorption of 3.54 per cent. of the heat passing through a column of air 90 centimeters long takes place; for coloured gases he finds the absorption greater; but all colourless gases he assumes show no marked divergence from the atmosphere.—*Poggendorff's Annalen*, xciv. p. 337.

Friday, June 17, 1859.

THE LORD WENSLEYDALE, Vice-President, in the Chair.

PROFESSOR FARADAY, D.C.L. F.R.S.

On Phosphorescence, Fluorescence, &c.

THE agent understood by the word "light," presents phenomena so varied in kind, and is excited to sensible action by such different causes, acting apparently by methods differing greatly in their physical nature, that it excites the hopes of the philosopher much in relation to the connexion which exists between all the physical forces, and the expectation that that connexion may be greatly developed by its means. This consideration, with the great advance in the experimental part of the subject which has recently been made by E. Becquerel, were the determining causes of the production of this subject before the members of the Royal Institution on the present occasion.

The well known effect of light in radiating from a centre, and rendering bodies visible which are not so of themselves, as long as the emission of rays was continual—the general nature of the undulatory view, and the fact that the mathematical theory of these assumed undulations was the same with that of the undulation of sound, and of any undulations occurring in elastic bodies, were referred to as a starting position. Limited to this effect of light it was observed that the illuminated body was luminous only whilst receiving the rays or undulations.

But superadded occasionally to this effect is one known as *phosphorescence*, which is especially evident when the sun is employed as the source of light. Thus, if a calcined oyster-shell, a piece of white paper, or even the hand, be exposed to the sun's rays and then instantly placed before the eyes in a perfectly dark room, they are seen to be visible *after* the light has ceased to fall on them. There is a further philosophical difference, which may be thus stated; if a piece of white oyster-shell be placed in the spectrum rays issuing from a prism, the parts will, as to illumination, appear red, or green, or blue, as they come under the red, green, or blue rays: whereas if the phosphorescent effect be observed, *i.e.* that effect remaining after the illuminating rays are gone, the light will either be white, or of a tint not depending upon the colour of the ray producing it, but upon the nature of the substance itself, and the same for all the rays.

The ray which comes to the eye in an ordinary case of visibility, may be considered as that which, emanating from the luminous body,

has impinged upon the substance seen, and has been deflected into a new course, namely towards the eye; it may be considered as the same ray, both before and after it has met with the visible body. But the light of phosphorescence cannot be so considered, inasmuch as *time* is introduced; for the body is visible for a time sensibly after it has been illuminated, which time in some cases rises up to minutes, and perhaps hours. This condition connects these phosphorescent bodies with those which phosphoresce by heat, as apatite and fluor-spar; for when these are made to glow intensely by a heat far below redness, it is evident that they have acquired a state which has enabled them for a time to become original sources of light, just as the other phosphorescent bodies have by exposure to light acquired a like state. And then again there is this further fact, that as the fluor spar which has been heated, does not phosphoresce a second time when reheated, still it may be restored to its first state by passing the repeated discharge of the electric spark over it, as Pearsall has shown.

Then follows on (in the addition of effect to effect) the phenomena of *fluorescence*, and the fine contributions to our knowledge of this part of light by Stokes. If a fluorescent body, as uranium glass, or a solution of sulphate of quinine, or decoction of horse-chestnut bark are exposed to diffuse day-light, they are illuminated, not merely abundantly but peculiarly, for they appear to have a glow of their own; and this glow does not extend to all parts of the bodies, but is limited to the parts where the rays first enter the substances. Some feeble flames, as that of hydrogen, can produce this glow to a considerable degree. If a deep blue glass be held between the body and the rays of the sun, or of the electric lamp, it seems even to increase the effect; not that it does so in reality, but that it stops very many of the luminous ray, yet lets the rays producing this effect pass through. By using the solar or electric spectrum, we learn that the most effectual rays are in most cases not the luminous ones, but are in the dark part of the spectrum; and so the fluorescence appears to be a luminous condition of the substance, produced by dark rays which are stopped or consumed in the act of rendering the fluorescent body luminous: so they produce this effect only at the first or entry surface, the passing ray, though the light goes onward, being unable to produce the effect again; and this effect exists only whilst the competent ray is falling on to the body, for it disappears the instant the fluorescent substance is taken out of the light, or the light shut off from it.

When E. Becquerel attacked this subject he enlarged it in every direction.* First of all, he prepared most powerful phosphori; these being chiefly sulphurets of the alkaline earths, strontia, baryta, lime. By treatment and selection he obtained them so that they would emit a special colour: thus, seven different tubes might contain preparations which exposed to the sun, or diffused day-light, or the electric light, should yield the seven rays of the spectrum. The light emitted

* Annales de Chimie et de Physique, 1859, tome Iv. p. 1.

generally possessed a lower degree of refrangibility than the ray causing the phosphorescence; but in some instances he was able to raise the refrangible character of the ray emitted to that of the exciting ray. By taking a given preparation, and raising it to different temperatures, he caused it to give out different coloured rays by the single action of one common ray; this variation in power returning to a common degree as the temperatures of the phosphori became the same in all. He showed that *time* was occupied in the elevation of the phosphorescent state by the ray; and also that time was concerned in various degrees during the emission of the phosphorescent ray: that this time, which in many cases was long, might be affected, being shortened by the action of heat, and then the brilliancy of the phosphorescence for the shortened time was increased. He showed the special relation of the different phosphori to the different rays of the spectrum, pointing out where the maximum effect occurred; also that there were the equivalents of dark bands, *i.e.* bands in the spectrum, where little or no phosphorescence was produced.

These phosphori were many of them highly fluorescent. Thus, if one of them was exposed to the strong voltaic light, and then placed in the dark, it was seen to be brilliantly luminous, gradually sinking in brightness, and ultimately fading away altogether: but if it were held in the rays beyond the violet end of the spectrum (the more luminous rays being shut off) it was again seen to be beautifully luminous, but that state disappeared the instant it was removed from the ray. Now this is fluorescence, and the same body seemed to be both phosphorescent and fluorescent. Considering this matter, and all the circumstances regarding time, Becquerel was led to believe that these two luminous conditions differed essentially only in the *time* during which the state excited by the exposure to light continued; that a body being really phosphorescent, but whose state fell instantly, was fluorescent, giving out its light while the exciting ray continued to fall on it, and during that time only; and that a phosphorescent was only a more sluggish, body, which continued to shine after the exciting ray was withdrawn. To investigate this point he invented the *phosphoroscope*; an apparatus which may vary in its particular construction, but in which discs or other surfaces illuminated by the sun or an electric lamp, might, by revolution, be rapidly placed before the eye in a dark chamber, and so be regarded in the shortest possible space of time after their illumination. By such an apparatus Becquerel showed that all the fluorescent bodies were really phosphorescent; but that the emission of light endured only for a very short time.

An extensive series of experimental illustrations upon the foregoing points was made with fine specimens of phosphori, for which the speaker was indebted to M. Becquerel himself. The phosphoroscope employed consisted of a cylinder of wood, one inch in diameter and seven inches long, placed in the angle of a black box with the electric lamp inside, so that three-fourths of the cylinder were external, and in the dark chamber where the audience sat, and one-fourth was within the box, and in the full power of the voltaic light. By proper mechanical arrange-

ments this cylinder could be revolved, and the part which was at one instant within, rapidly brought to the outside, and observed by the audience. As the cylinder could be made to revolve 300 times in a second, and as the twentieth part of a revolution was enough to bring a sufficient portion of the cylinder to the outside, it is evident that a phosphorescent effect which would last only the 1-3000th or even the 1-6000th of a second might be made apparent. All escape of light between the moving cylinder and the box was prevented by the use of properly attached black velvet.

The cylinder was first supplied with a surface of Becquerel's phosphori. The effect here was, that when by rotation the part illuminated was brought outside the box it was found phosphorescent. If the cylinder continued to rotate it appeared equally luminous all over, and when the rotation ceased, or the lamp was extinguished, the light gradually sank as the phosphorescence fell. Then a cylinder having a surface of quinine or æsculin was put into the apparatus. Whilst the cylinder was still it was dark outside; but when revolving with moderate velocity it became luminous outside, ceasing to be so the moment the revolution stopped. Here the fluorescence was evidently shown to occupy time; indeed, the full time of a revolution: and taking advantage of that, the self-shining of the body was separated from its illumination within, and the fluorescence made to assume the character of phosphorescence. Another cylinder was covered with crystals of nitrate of uranium, a hot saturated solution having been applied over it with a fine brush. The result was beautiful. A moderate degree of revolution brought no light out of the box; but with increased motion it began to appear at the edge. As the rapidity became greater, the light spread over the cylinder, but it could not be carried over the whole of its surface. It issued as a band of light where the moving cylinder left the edge of the box, diminishing in intensity as it went on, and looking like a bright flame, wrapping round half the cylinder. When the direction of revolution was reversed, this flame issued from the other side; and when the motion of the cylinder was stopped, all the phenomena of fluorescence or phosphorescence disappeared at once. The wonderfully rapid manner in which the nitrate of uranium received the action of the light within the box, and threw off its phosphorescence outside, was beautifully shown.

The electric light, even when the discharge is in rarefied media, or as a feeble brush, emits a great abundance of those rays, which produce the phenomena of fluorescence; but then if these rays have to pass through common glass they are cut off, being absorbed and destroyed even when they are not expended in producing fluorescence or phosphorescence. Arrangements can however be made in which the advantageous circumstances can be turned to good account with such bodies as Becquerel's phosphori or uranium glass. If these be enclosed within glass tubes, having platinum wires at the extremities, and which are also exhausted of air and hermetically sealed, then the discharges of a Ruhmkorff coil can be continually sent over the phosphori, and the effects both fluorescent and phosphorescent be beauti-

fully shown. The first or immediate light of the body is often of one colour, whilst on the cessation of the discharge the second or deferred light is of another; and many variations of the effects can be produced.

In connexion with rarefied media it may be remarked, that some of the tubes by Geissler and others have been observed to have their rarefied atmospheres phosphorescent, glowing with light for a moment or two after the discharge through them was suspended. Since then Becquerel has observed that oxygen is rendered phosphorescent, *i.e.* that it presents a persistent effect of light, when electric discharges are passed through it. I have several times had occasion to observe that a flash of lightning, when seen as a linear discharge, left the luminous trace of its form on the clouds, enduring for a sensible time after the lightning was gone. I strictly verified this fact in June, 1857, recording it in the "Philosophical Magazine,"* and referred it to the phosphorescence of the cloud. I have no doubt that that is the true explanation. Other phenomena, having relation to fluorescence and phosphorescence, as the difference in the light of oxygen and hydrogen exploded in glass globes, or in the air, were referred to, with the expression of strong hopes that Becquerel's additions to that branch of science would greatly explain and extend them.

[M. F.]

* Philosophical Magazine, June, 1857, p. 506.

Friday, January 20, 1860.

SIR BENJAMIN COLLINS BRODIE, BART. President of the Royal Society,
Vice-President R.I. in the Chair.

JOHN TYNDALL, ESQ. F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY IN THE ROYAL INSTITUTION.

On the Influence of Magnetic Force on the Electric Discharge.

THE intention of the speaker was to bring before the meeting a series of experiments illustrative of the constitution of the electric discharge and of the action of magnetism upon it. The substance of the discourse was derived from the researches of various philosophers, its form being regulated to suit the requirements of the audience.

1. The influence of the transport of particles was first shown by an experiment suggested, it was believed, by Sir John Herschel, and performed by Professor Daniell. The carbon terminals of a battery of 40 cells of Grove were brought within one-eighth of an inch of each other, and the spark from a Leyden jar was sent across this space. This spark bridged with carbon particles the gap which had previously existed in the circuit, and the brilliant electric light due to the passage of the battery current was immediately displayed.

2. The magnified image of the coal points of an electric lamp was projected upon a white screen, and the distance to which they could be drawn apart without interrupting the current was noted. A button of pure silver was then introduced in place of the positive carbon, a luminous discharge four or five times the length of the former being thus obtained. The silver was first observed to glow, and afterwards to pass into a state of violent ebullition. A narrow dark space was observed to surround one of the poles, corresponding probably with the dark space observed in the discharge of Ruhmkorff's coil through rarefied media.*

3. The action of a magnet upon the splendid stream of green light obtained in the foregoing experiment was exhibited. A small horseshoe magnet of Logemann was caused to approach the light, which was bent hither and thither, according as the poles of the magnet changed their position: the discharge in some cases formed a magnificent green bow, which on the further approach of the magnet was torn asunder, and the passage of the current thereby interrupted. It was Davy who first showed the action of a magnet upon the voltaic arc. The

* Mr. Faraday noticed this dark stripe while the speaker was making his preparatory experiments.

transport of matter by the current was further illustrated by a series of deposits on glass obtained by Mr. Gassiot from the continued discharge of an induction coil.

4. A discharge from Ruhmkorff's coil was sent through an attenuated medium; and the glow, which surrounded the negative electrode was referred to. One of the most remarkable effects hitherto observed was that of a magnet upon this negative light. Plücker had shown that it arranges itself under the influence of the magnet exactly in the direction of the magnetic curves. Iron filings strewn in space, and withdrawn from the action of gravity, would arrange themselves around a magnet exactly in the manner of the negative light.

An electric lamp was placed upon its back; a horseshoe magnet was placed horizontally over its lens, and on the magnet a plate of glass: a mirror inclined at an angle of 45° received the beam from the lamp, and projected it upon the screen. Iron filings were scattered on the glass, and the magnetic curves thus illuminated were magnified, and brought to clear definition upon the screen. The negative light above referred to arranges itself, according to Plücker, in a similar manner.

5. The rotation of an electric current round the pole of a magnet, discovered by Mr. Faraday in the Royal Institution, nearly forty years ago, was next shown; and the rotation of a luminous current from an induction coil in an exhausted receiver by the same magnet was also exhibited, and both shown to obey the same laws. This beautiful experiment was devised by De la Rive.

6. Into a circuit of 20 cells a large coil of copper wire was introduced, and when the current was interrupted, a bright spark, due to the passage of the extra current, was obtained. The brightness and loudness of the spark were augmented when a core of soft iron was placed within the coil. The disruption of the current took place between the poles of an electro-magnet; and when the latter was excited, an extraordinary augmentation of the loudness of the spark was noticed. This effect was first obtained by Page, and was for a time thought to denote a new property of the electric current.

But Rijke had shown in a paper, the interest of which is by no means lessened by the modesty with which it is written, that the effect observed by Page is due to the sudden extinction of the primary spark by the magnet; which suddenness concentrates the entire force of the extra current into a moment of time. Speaking figuratively, it was the concentration of what, under ordinary circumstances, is a mere push, into a sudden kick of projectile energy.

7. The contact-breaker of an induction coil was removed, and a current from five cells was sent through the primary wire. The terminals of the secondary wire being brought very close to each other, when the primary was broken by the hand, a minute spark passed between the terminals of the secondary. When the disruption of the primary was effected between the poles of an excited electro-magnet, the small spark was greatly augmented in brilliancy. The terminals

were next drawn nearly an inch apart. When the primary was broken between the excited magnetic poles, the spark, from the secondary jumped across this interval, whereas it was incompetent to cross one-fourth of the space when the magnet was not excited. This result was also obtained by Rijke; who rightly showed, that in this case also the augmented energy of the secondary current was due to the augmented speed of extinction of the primary spark between the excited poles. This experiment illustrated in a most forcible manner the important influence which the mode of breaking contact may have upon the efficacy of an induction coil.

The splendid effects obtained from the discharge of Ruhmkorff's coil through exhausted tubes were next referred to. The presence of the coil had complicated the theoretic views of philosophers, with regard to the origin of those effects; the intermittent action of the contact-breaker, the primary and secondary currents, and their mutual reactions, producing tertiary and other currents of a higher order, had been more or less invoked by theorists, to account for the effects observed. Mr. Cassiot was the first to urge, with a water battery of 3500 cells, a voltaic spark across a space of air, *before* bringing the electrodes into contact: with the self-same battery he had obtained discharges through exhausted tubes, which exhibited all the phenomena hitherto observed with the induction coil. He thus swept away a host of unnecessary complications which had entered into the speculations of theorists upon this subject.

8. On the present occasion, through the kindness of Mr. Cassiot, the speaker was enabled to illustrate the subject by means of a battery of 400 of Grove's cells. The tension at the ends of the battery was first shown by an ordinary gold-leaf electroscope; one end of the battery being insulated, a wire from the other end was connected with the electroscope; the leaves diverged; on now connecting the other end of the battery with the earth, the tension of the end connected with the electrometer rose, according to a well-known law, and the divergence was greatly augmented.

9. A large receiver (selected from Mr. Cassiot's fine collection), in which a vacuum had been obtained by filling it with carbonic acid gas, exhausting it, and permitting the residue to be absorbed by caustic potash, was placed equatorially between the poles of the large electro-magnet. The jar was about six inches wide, and the distance between its electrodes was ten inches. The negative electrodes consisted of a copper dish, four inches in diameter, the positive one was a brass wire.

On the 16th of this month an accident occurred to this jar. Mr. Faraday, Mr. Cassiot, and the speaker had been observing the discharge of the nitric acid battery through it. Stratified discharges passed when the ends of the battery were connected with the electrodes of the receiver; and on one occasion the discharge exhibited an extraordinary effulgence; the positive wire emitted light of dazzling brightness, and finally gave evidence of fusion. On interrupting the circuit,

the positive wire was found to be shortened about half an inch, its metal having been scattered by the discharge over the interior surface of the tube.

10 The receiver in this condition was placed before the audience in the position mentioned above. When the ends of the 400-cell battery were connected with the wires of the receiver, *no discharge passed*; but on touching momentarily with the finger any portion of the wire between the positive electrode of the receiver and the positive pole of the battery, a brilliant discharge instantly passed, and continued as long as the connexion with the battery was maintained. This experiment was several times repeated: the connexion with the ends of the battery was not sufficient to produce the discharge, but in all cases the touching of the positive wire caused the discharge to flash through the receiver.

Previous to the fusion of the wire above referred to, this discharge usually exhibited fine stratification: its general character now was that of a steady glow, through which, however, intermittent luminous gushes took place, each of which presented the stratified appearance.

11. On exciting the magnet between whose poles the receiver was placed, the steady glow curved up or down according to the polarity of the magnet, and resolved itself into a series of effulgent transverse bars of light. These appeared to travel from the positive wire along the surface of the jar. The deflected luminous current was finally extinguished by the action of the magnet.

12. When the circuit of the magnet was made and immediately interrupted, the appearance of the discharge was extremely singular. At first the strata rushed from the positive electrode along the upper surface of the jar, then stopped, and appeared to return upon their former track, and pass successively with a deliberate motion into the positive electrode. They were perfectly detached from each other; and their successive engulfments at the positive electrode were so slow as to be capable of being counted aloud with the greatest ease. This deliberate retreat of the strata towards the positive pole was due, no doubt, to the gradual subsidence of the power of the magnet. Artificial means might probably be devised to render the recession of the discharge still slower. The rise of power in the magnet was also beautifully indicated by the deportment of the current.

After the current had been once quenched, as long as the magnet remained excited, no discharge passed: but on breaking the magnet circuit, the luminous glow reappeared. Not only then is there an action of the magnet upon the particles transported by an electric current, but the above experiment indicates that there is an action of the magnet upon the electrodes themselves, which actually prevents the escape of their particles. The influence of the magnet upon the electrode would thus appear to be *prior* to the passage of the current.

13 The discharge of the battery was finally sent through a tube,

whose platinum wires were terminated by two small balls of carbon : a glow was first produced ; but on heating a portion of the tube containing a stick of caustic potash, the positive ball sent out a luminous protrusion, which subsequently detached itself from the ball ; the tube becoming instantly afterwards filled with the most brilliant strata. There can be no doubt that the superior effulgence of the bands obtained with this tube is due to the character of its electrodes : *the bands are the transported matter of these electrodes*. May not this be the case with other electrodes ? There appears to be no uniform flow in nature ; we cannot get either air or water through an orifice in a uniform stream ; the friction against the orifice is overcome by starts, and the jet issues in pulsations. Let a lighted candle be quickly passed through the air ; the flame will break itself into a beaded line in virtue of a similar intermittent action, and it may be made to sing, so regular are the pulses produced by its passage. Analogy might lead us to suppose that the electricity overcomes the resistance at the surface of its electrode in a similar manner, escaping from it in tremors ; the matter which it carries along with it being broken up into strata, as a liquid vein is broken into drops.*

[J. T.]

* Mr. Gassiot has shown that a *single discharge* of the Leyden jar produces the stratification. May not every such discharge correspond to a single draw of a violin bow across a string ?

Friday, March 2, 1860.

SIR HENRY HOLLAND, BART. M.D. F.R.S. Vice-President,
in the Chair.

PROFESSOR H. E. ROSCOE,

On the Measurement of the Chemical Action of the Solar Rays.

THOSE portions of the solar rays which vibrate most slowly, and are situated near the red end of the spectrum, are those which mainly regulate the alterations of temperature on the surface of our planet. They are, *par excellence*, the heating rays. They principally produce all those motions in our atmosphere which we term winds; they effect those grand phenomena of distillation and deposits which we call rains; and the amount and distribution of those heating rays at any point on the earth's surface determines the thermal climate of that point.

On a scale, perhaps less grand, but certainly not less important as regards their effects, are the actions produced by the most rapidly vibrating portion of the sun's rays; those, namely, which are situated near the violet end of the spectrum. These rays have been called the chemical rays, because it is by these especially that the chemical action of the sunlight is effected. It is in presence of these rays alone that the plant is enabled to decompose the carbonic acid of the air, to assimilate the carbon, restoring the oxygen for the subsequent use of animals. Hence the amount and distribution of these rays at any given place regulates to a great extent the character of the fauna and flora; gives, in short, the "chemical climate" of the place.

The measurement of the quantity of this solar energy, falling at any time on a given spot upon the earth's surface, must be a subject of primary importance in the determination of the physical history of our globe. We fortunately possess a method, although it is only a comparative one, for measuring the amount of effect which the heating rays produce, that is for measuring *temperature*. No such mode of measurement for those of the solar rays which especially effect chemical action has, up to the present time, been adopted: not that meteorologists have ignored the importance of the subject, but because the difficulties which beset the establishment of a measuring instrument for chemical action were considered to be insurmountable.

The speaker remarked, that his object was to bring before his audience the principles and mode of action of a method employed for the measurement of the chemical action of light.*

As an illustration of the chemical action of light, attention was directed to the fact, that when a perfectly pure mixture of exactly equal volumes of chlorine and hydrogen gases is exposed to light, the gases combine, producing an equal volume of hydrochloric acid gas, whilst no such combination occurs in the dark. This combination may occur gradually, or with great rapidity. If the chemical activity of the light be great, the union takes place quickly, great heat is evolved, a sudden expansion takes place, and the vessel containing the mixture of chlorine and hydrogen is shattered by the explosion. The gradual or slow combination may be rendered evident by allowing the hydrochloric acid thus formed to be absorbed by water; the consequent diminution of bulk of the gas accurately representing the chemical action effected.

This mixture of equal volumes of chlorine and hydrogen is used as the sensitive substance for measuring the chemical action of light. It is evolved in the perfectly pure state by the electrolytic decomposition of strong aqueous hydrochloric acid; and it is by this method only, that it can be prepared. The gases thus evolved are in the exact proportion in which they exist in hydrochloric acid; so that, if by any means, we re-combine these gases, no trace of either substance will remain behind, the whole uniting to form hydrochloric acid.

For the purpose of measuring this chemical action, effected, not only by solar light, but also by light from many artificial sources, we require some instrument, which is to the chemical action of light what the thermometer is to the heat actions; an instrument which will show objectively the amount of chemically active light. We must be sure, in the first place, that our mode of measurement is a reliable one. That, as in the case of the thermometer, equal increments of volume, correspond to equal increments of heat, so, in the new instrument, the indications, however obtained, shall be proportional to, and represent the amount of chemical rays emanating from, any source.

This has been accomplished in the chemical photometer; by the help of which an accurate measurement of the chemical action of light is effected.

The facts upon which this mode of measurement is based, may be summed up as follows:

1. Exactly equal volumes of chlorine and hydrogen gases, when mixed, combine together on exposure to light, forming hydrochloric acid gas.
2. This combination does not occur in the dark.

* For a detailed description of apparatus, &c. see "Photochemical Researches," Part I. "Measurement of the Chemical Action of Light," by R. Bunsen and H. E. Roscoe.—*Phil. Trans.* 1857, p. 355.

3. The quantity of hydrochloric acid thus formed is directly proportional to the intensity of the incident light, and serves, therefore, as a measure of the chemical action produced.
4. The chemical photometer is an instrument, by help of which the quantity of hydrochloric acid thus formed, can be accurately measured.

The chemical photometer consists essentially of three parts ; namely, first, the apparatus in which the sensitive gas is generated ; secondly, the apparatus in which the gas is exposed to the light ; and thirdly, the apparatus in which the volume of hydrochloric acid produced in a given time is read off.

When very numerous precautions in the management of the photometer are taken, it proves a most sensitive and reliable instrument. Having thus obtained an instrument by which the chemical action of light can be accurately measured, it only remains to graduate it. For this purpose we require a standard of light, from which the determination is to proceed. For this comparative measurement, the possession of a constant source of light is the first essential. This is obtained as follows :—

1. A flame of pure carbonic oxide gas, burning in the air and issuing from an opening of given size at a given rate, is employed as the *standard flame*.
2. *The unit amount of chemical action*, is that effected by such a flame upon the sensitive mixture of chlorine and hydrogen during one minute, at the distance of one metre.
3. The quantity of chemically active light producing this action is called *one chemical unit of light*; and ten thousand of such units *one chemical degree of light*.
4. The chemical photometer is graduated by observing how many of these chemical units of light correspond to one division on the scale of the instrument.

As an illustration of the mode in which this measurement of the chemical action of light is employed, the speaker described the method by which the chemical action produced by the direct solar rays has been determined.* For this purpose, it was necessary to admit a very small, but a known, portion of direct sun-light into the dark room in which the instrument was placed, and to allow the insolation vessel to be bathed in the pencil of rays thus admitted. By help of Silbermann's heliostate, the sun's image was reflected during the whole day upon one spot, a small opening of known size, in the window shutter of a dark room. The fraction of the total sun's rays thus admitted and allowed to fall upon the chemical photometer can be calculated, and the action thus effected, observed ; hence the amount

* The full memoir on this subject is to be found in Poggendorff's *Annal.* Bd. cviii. p. 193. In *Abstract, Proceedings Royal Society*, Vol. x. p. 39, 1859. *Photochemical Researches*, Part 4, by R. Bunsen and H. E. Roscoe.

of action can be found which the sun would have produced if directly shining upon the instrument ; a condition, impossible of course to fulfil, as the action would become too rapid and the whole apparatus would be shattered by explosion.

The day chosen for observation of the sun's action must obviously be cloudless, if we wish to obtain an idea of the relation existing between the chemical action and the height of the sun. Beginning the observations as near sunrise as possible, we find, for instance, on September 15th, 1858, one of the days on which such a series of experiments was made, that at 7^h. 9^m. a.m., when the sun's zenith distance was 76° 30', the observed action amounted to 1.52. That is, in one minute the column of water moved through 1.52 division ; or the quantity of hydrochloric acid formed, when the sun stood at the height mentioned, was represented by 1.52 division on the scale.

Gradually, as the day wore on, the observed action for each minute became larger ; until at 9^h. 14^m. a.m., the latest observation possible on the day in question, owing to the formation of clouds, the action reached 18.5 divisions, or was thirteen times as large as at 7^h. 9^m. In the last column of the accompanying table is found the action, expressed in degrees of light, which would have been observed at the foregoing times, if the whole sunlight had been allowed to fall on the instrument.

TABLE I.

Hour.	Sun's Zenith. Distance.	Observed Action. 1 Minute.	Total Sun's Action in Degrees of Light.
7 ^h 9 ^m	76° 30'	1.52	5.54
7 26	73° 49'	4.22	15.50
7 40	71° 37'	6.09	22.43
8 0	68° 34'	7.56	27.85
8 7	67° 30'	8.38	38.87
8 26	64° 42'	12.48	45.85
8 54	60° 48'	17.09	62.59
9 14	58° 11'	18.51	67.61

This great increase in the chemical action with the rise of the sun in the heavens simply results from the fact that the solar rays, in passing through the air, are extinguished or absorbed, lost in fact as light ; and that as the sun rises higher above the horizon, the column of air through which the rays pass is constantly being lessened ; consequently more of the direct rays reach the earth.

Now, the law according to which the direct rays of the sun are thus absorbed in the air can be obtained from the experiments, of which the foregoing is only an example ; hence, if the action which the sun produces, when at a given height, is known, it is possible to calculate the action which it would produce at any other height.

That these calculated results agree very closely with the experimental data,—with the observed action,—is seen by comparing the numbers in Table, No. II., expressing the observed and calculated action.

TABLE II.

The amount of Chemical Action effected at a point upon the Earth's Surface on any cloudless day, by the direct Solar Rays, depends alone upon the Sun's zenith distance; or upon the height of the column of air through which the Rays have to pass.

Sun's Zenith Distance at time of Observation.	Chemical Illumination of Sun's Direct Rays at the Earth's Surface expressed in degrees of Light.	
	Observed.	Calculated.
46° 8'	93·0	96·4
50° 51'	89·2	85·8
57° 35'	63·1	67·9
58° 11'	67·6	66·2
60° 48'	62·6	58·3
64° 42'	45·9	47·9
67° 30'	38·9	36·6
68° 34'	27·9	33·1
71° 37'	22·4	24·5
73° 49'	15·5	16·3
76° 30'	5·5	9·2

Probable error = ± 2.7 degrees of Light.

Knowing the law which regulates the absorption of the chemical rays, we can calculate what the action would be if there were no atmosphere to diminish the power of the rays. It is thus found that if the sun's rays were not thus weakened, by passage through the atmosphere, they would produce an illumination represented by 318 *degrees of light*: or they would effect a combination in one minute, upon an unlimited atmosphere of chlorine and hydrogen on which they fell perpendicularly, of a column of hydrochloric acid, 35·3 *metres in height*. The sun's rays having passed perpendicularly through our atmosphere to the sea's level, effect an action of only 14·4 *light metres*; or nearly two-thirds of their chemical activity has been lost by extinction and dispersion in the atmosphere.

A large number of most interesting conclusions may be drawn from the facts already noticed. Thus, for instance, we may determine the chemical action which the solar rays will produce on the various planets; for we know that the intensity of the chemical illumination varies inversely as the square of the distance of the planet from the sun. The numbers in Table III., express this chemical action in degrees of light, and in heights of columns of hydrochloric acid called

light metres. Hence, we see how much the sun's chemical action varies on the different planets; the superior planets receiving so small a portion as to render it impossible that the kind of animal and vegetable life which we here enjoy can there exist.

TABLE III.—*Chemical Action produced by Direct Sunlight on each Planet.*

	Mean Distances.	Chemical Action in	
		Light Degrees.	Light Metres.
Mercury . . .	0·387	2125·0	235·1
Venus . . .	0·723	608·9	67·5
Earth . . .	1·000	318·3	35·3
Mars . . .	1·524	137·1	15·2
Jupiter . . .	5·203	11·8	1·2
Saturn . . .	9·539	3·5	0·4
Uranus . . .	19·183	1·0	0·1
Neptune . . .	30·040	0·4	0·04

Interesting conclusions can be drawn from these facts, concerning the distribution of the chemical rays on the surface of our earth in different latitudes, and at different elevations above the sea's level. The farther removed a situation is from the level of the sea, the higher up in the atmosphere it is placed, the greater amount of chemical action it will receive. Thus, in the highlands of Thibet, where corn and grain flourish at a height of from 12,000 to 14,000 feet, the chemical action of the direct sunlight is $1\frac{1}{2}$ times as great as in the neighbouring lowland plains of Hindostan. In the same way we can calculate for any point of the earth's surface whose latitude is known, the amount of chemical action which the direct sunlight effects at any given time of day or year. In Table IV. the numbers represent the chemical

TABLE IV.—*Chemical Action effected by Direct Sunlight in One Minute on the Vernal Equinox at*

A. Melville Island. B. Reikiavik, Iceland. C. St. Petersburg. D. Manchester.				E. Heidelberg. F. Naples. G. Cairo.			
Hour.	A.	B.	C.	D.	E.	F.	G.
6 a.m. or 6 p.m.	0·0	0·0	0·0	0·0	0·0	0·0	0·0
7 " 5	0·0	0·02	0·07	0·22	0·38	0·89	1·74
8 " 4	0·07	1·53	2·88	5·85	8·02	13·31	20·12
9 " 3	0·67	6·62	10·74	18·71	23·99	35·88	50·01
10 " 2	1·86	13·27	20·26	32·91	40·94	58·46	78·61
11 " 1	3·02	18·60	27·55	43·34	53·19	74·37	98·33
12 at noon	3·51	20·60	30·26	47·15	57·62	80·07	105·3

action effected by direct sunlight in one minute at the places and hours named on the 21st of March. Curves were exhibited, showing the rise of the action, with the progress of the sun through the heavens. By comparing the numbers in the table, it is seen how greatly this chemical action differs at various points on the earth's surface; and we can understand how it is, that at the latitude of Cairo, where the chemical action of the direct sunlight is twice as great as it is in that of Manchester, the whole flora and fauna assume a more tropical and luxuriant character.

The speaker stated, that he was only able briefly to notice the principles upon which the new mode of measuring the chemical action of light depends; adding one or two illustrations of the measurements actually made. He was unable even to refer to one of the most interesting and important applications, viz, the measurement of the chemical action effected by the diffuse daylight. This has, however, been accomplished, and we are now able to calculate the amount of chemical action produced by both diffused and direct solar light, on a cloudless day, at any place situated above the latitude of Cairo. The following table shows the results of such a calculation.

TABLE V.—*Total Chemical Action effected by the Solar Rays from Sunrise to Sunset on the Vernal Equinox, at*

	I.	II.	III.	IV.
Melville Island . . .	1196	10590	11790	1306
Rejkiavik . . .	5964	15020	20980	2324
St. Petersburg . . .	8927	16410	25340	2806
Manchester . . .	14520	18220	32740	3625
Heidelberg . . .	18240	19100	37340	4136
Naples . . .	26640	20550	47190	5226
Cairo . . .	36440	21670	58110	6437

I.	Gives the action of direct sunlight in degrees of light.			
II.	"	diffuse daylight	"	
III.	"	total light	"	
IV.	"	"	in light metres.	

Knowing the intimate connection of the chemically active solar rays with the plant and animal-producing power of a country, no one can doubt the immense importance of the establishment of a regular series of measurements of the variations of the amount of these chemical rays at different situations on the earth's surface. Such a series would open an entirely new field to the science of meteorology, and would reveal to us relations and points of difference as regards the chemical climate, at present wholly unknown.

The chemical photometer, in the hands of an experienced experimenter, is a perfectly accurate and extremely sensitive instrument; and the method described is a most valuable one for scientifically investi-

gating the primary laws regulating the chemical action of light, and the distribution of the chemical rays. It is, however, not capable of universal application as a meteorological instrument, owing to its complicated nature, and the great care requisite in its management. At present we know of no easy, and at the same time correct, method of estimating the chemical action of light. Much time and labour has already been spent by the authors of the method described in endeavouring to prepare an instrument, which can be practically used for this purpose in meteorological observations. Persevering in their efforts, they hope ere long to overcome the numerous difficulties which beset the subject, and to describe a method which shall answer the proposed end.

[H. E. R.]

Friday, March 30, 1860.

THE LORD WENSLEYDALE, Vice-President, in the Chair.

WILLIAM ODLING, Esq. M.B. F.R.S.

SECRETARY TO THE CHEMICAL SOCIETY.

On Acids and Salts.

It is natural to inquire whether the doctrines of series and substitutions, which are essential for the association of organic products, may not throw some additional light upon the simpler compounds of mineral chemistry, when viewed as unitary molecules; and particularly upon the relations and properties of the mineral acids and their salts, which have hitherto constituted the strongholds of the electro-chemical, or binary, theory of combination.

The doctrine of series affirms that chemical compounds may be arranged in series, the successive members of each of which differ from one another in composition by a common increment, and are associated with one another by a certain relation of properties, the exact nature of the relation varying with the nature of the increment.

The doctrine of substitutions affirms that, in very many chemical compounds, one or more atoms may be displaced by some other atoms or groupings, and that the new bodies, resulting from this displacement, correspond in constitution with the normal bodies from which they were derived. The doctrine of substitutions affords great assistance to the doctrine of series; for when, as frequently happens, a gap exists in any series, that gap can almost always be filled up by a substitution-representative of the missing body.

(*α.*) There are four acid compounds of hydrogen, two volumes of each of which contain one volume of hydrogen, namely:

HF	Fluorhydric ac.
HCl	Chlorhydric acid.
HBr	Bromhydric acid.
HI	Iodhydric acids.

When two volumes of chlorhydric acid, for instance, are acted upon by a red hot iron-wire, the chlorine is absorbed by the iron, and one volume of hydrogen gas liberated. The two volumes of chlorhydric acid yield one volume of hydrogen, or the original bulk of gas is reduced to one-half by the absorption of its chlorine. The above four acids may be looked upon as substitution-representatives, one of another.

Chlorhydric acid yields the following series of oxides, convertible into each other by mutual metamorphosis.

HCl	Chlorhydric acid.
HClO	Hypochlorous acid.
HClO ₂	Chlorous acid.
HClO ₃	Chloric acid.
HClO ₄	Perchloric acid.

When chlorhydric acid HCl, is oxidated by permanganic acid, hypochlorous acid HClO, is produced; and, conversely, chlorhydric acid may be reproduced by the deoxidation of hypochlorous acid. Hypochlorous acid, when heated, breaks up into chloric acid HClO₃, and other products. When chloric acid is deoxidated by nitrous acid, it becomes chlorous acid HClO₂; and, when oxidated at the positive pole of a galvanic battery, it becomes perchloric acid HClO₄. Here then is a series of associated acids, expressed as unitary molecules, by the simplest possible formulæ, and arranged in a series, the successive members of which differ from one another in composition by an increment of one atom, or volume, of oxygen.

(β.) There are four other binary compounds of hydrogen, two volumes of each of which, however, contain two volumes of hydrogen, namely:

H ₂ O	Water.
H ₂ S	Sulphydric acid.
H ₂ Se	Selenhydric acid.
H ₂ T	Tellurhydric acid.

A given volume of any one of these gases or vapours contains exactly twice the quantity of hydrogen, that the same volume of any one of the first class of gases contains. When two volumes of sulphydric acid, for instance, are acted upon by a red hot iron-wire, the sulphur is absorbed by the iron, and two volumes of hydrogen gas are liberated. The two volumes of sulphydric acid yield two volumes of hydrogen, or the abstraction of the sulphur produces no alteration in the bulk of gas. The bihydric character of water, moreover, is well shown by the experiment of its electrolytic decomposition, in which two volumes of hydrogen are produced for every one volume of oxygen.

In the sulphur series of oxygen acids we have two gaps, which, however, can be filled up by the chloro-representatives of the missing bodies, thus:

H_2S	Sulphydric acid.	Cl_2S .
H_2SO	Wanting.	Cl_2SO .
H_2SO_2	Wanting.	Cl_2SO_2 .
H_2SO_3	Sulphurous acid.	
H_2SO_4	Sulphuric acid.	

The compounds Cl_2SO , and Cl_2SO_2 , are obtainable from the chloro-representative of sulphydric acid Cl_2S , by successive oxidation. The first product actually afforded by the oxidation of sulphydric acid is sulphurous acid H_2SO_3 , which is produced by the combustion of sulphydric acid in air or oxygen. Conversely, sulphydric acid may be obtained by deoxidating sulphurous acid with nascent hydrogen. Sulphuric acid H_2SO_4 , results from the oxidation of sulphurous acid, and by deoxidation can reproduce that body, as in the ordinary process for the preparation of sulphurous acid. Here then, including the chloro-representatives, is a second series of acids associated with one another by a common increment of composition, and by mutual metamorphosis.

Sulphuric acid H_2SO_4 , is the representative on the sulphur series, of perchloric acid HClO_4 , on the chlorine series. Each contains one atom of the radicle which gives the special character to the acid, in the one case chlorine, in the other sulphur. Each contains also four atoms, or volumes, of oxygen; but whereas perchloric acid contains only one atom, or volume, of hydrogen, sulphuric acid contains two atoms, or two volumes. And this difference in composition leads to a marked difference in the properties of the two acids. Perchloric acid HClO_4 , has only one atom of hydrogen that can be replaced. Hence it forms only one description of salt, such, for instance, as perchlorate of potassium KClO_4 , and only one description of ether, such, for instance as perchloric ether EtClO_4 . But sulphuric acid has two hydrogen atoms that can be replaced. Hence it can form acid salts, neutral salts, double salts, acid ethers, neutral ethers, double ethers, and saline ethers, as shown in the table.

H_2SO_4	Sulphuric acid.
KH SO_4	Acid sulphate of potassium.
K_2SO_4	Neutral sulphate of potassium.
KNi SO_4	Potassio-sulphate of nickel.
EtH SO_4	Ethylo-sulphuric acid.
Et_2SO_4	Neutral sulphate of ethyl.
EtMe SO_4	Ethylo-sulphate of methyl.
EtK SO_4	Ethylo-sulphate of potassium.

This property of forming acid and double salts, and acid and double ethers, &c., indicates a fundamental difference in character between sulphuric and perchloric acids, a difference that is satisfactorily represented by the difference in their formulæ as here written

down, HClO_4 , and H_2SO_4 . Bibasic characters are manifested as decidedly by the sulphurous and sulphydric acids.

(γ .) There are four other binary compounds of hydrogen, two volumes of each of which, however, contain three volumes of hydrogen, namely :

H_2N	Ammonia.
H_2P	Phosphamine.
H_2As	Arsenamine.
H_2Sb	Stibamine.

When the two volumes of phosphamine, for instance, are acted upon by a red hot iron-wire, the phosphorus is absorbed by the iron, and three volumes of hydrogen gas are liberated. Two volumes of chlorhydric acid yield one volume of hydrogen; two volumes of sulphydric acid yield two volumes of hydrogen, while two volumes of phosphamine yield three volumes of hydrogen; and this is a most important distinction between the three classes of hydrides to which these three gases respectively belong. Again, two volumes of gaseous ammonia, when decomposed by the Ruhmkorff spark, become converted into three volumes of hydrogen and one volume of nitrogen; or the original bulk of the ammonia becomes doubled.

In the phosphorus series of oxygen acids there is but one gap, and this can be filled up by the chlorine-, or the ethyl- representative of the missing body.

H_2P	Phosphamine	Cl_2P	Et_2P
H_2PO	Wanting	Cl_2PO	Et_2PO
H_2PO_2	Hypophosphorous acid		
H_2PO_3	Phosphorous acid		
H_2PO_4	Phosphoric acid		

Brodie has ascertained that oxichloride of phosphorus Cl_2PO , may be obtained directly by passing oxygen gas through boiling terchloride of phosphorus, or trichloro-phosphamine Cl_3P . The union of tri-ethyl phosphine Et_3P , with oxygen, to form the oxide of tri-ethylphosphine Et_3PO , constituted one of Hofmann's earliest experiments on the phosphorus bases. Proceeding to the actual oxides of phosphamine, it is doubtful whether hypophosphorous acid H_2PO_2 , has been obtained by the oxidation of phosphamine; but, on the other hand, phosphamine is readily obtainable by deoxidating hypophosphorous acid with nascent hydrogen; while by oxidating hypophosphorous acid, phosphorous and phosphoric acids are successively produced. Phosphorous acid H_2PO_3 , results from the slow oxidation, and phosphoric acid H_2PO_4 , from the rapid oxidation of phosphamine. Conversely, phosphamine may be obtained by the deoxidation of each of the two last-mentioned acids. Here again then is a series of naturally associated and mutually convertible bodies, represented by the simplest possible

formulae, by formulae which do not express any speculative view whatever, but merely indicate the indisputable fact that these bodies, or their representatives, differ from one another in composition, by the successive increments of one, two, three, and four oxygen atoms.

Phosphoric acid H_3PO_4 , is the representative on the phosphorus series, of sulphuric acid H_2SO_4 , on the sulphur series, and of perchloric acid HClO_4 , on the chlorine series; but whereas perchloric acid contains only one atom of hydrogen, and can form only one class of salts and ethers; whereas sulphuric acid contains only two atoms of hydrogen, and can form only two classes of salts and ethers; phosphoric acid contains three atoms of hydrogen, and can form three classes of salts and ethers. One-third, two-thirds, or three-thirds of its hydrogen may be displaced by a metal or basic radicle, or the hydrogen may be partly or wholly displaced by two or three different metals, or by two or three different radicles, or by a mixture of metals and radicles, thus: EtKC_2PO_4 , or $\text{H}(\text{NH}_4)\text{NaPO}_4$, &c.

(δ .) There is yet another primary hydride to be considered, namely, that of silicon, the siliciuretted hydrogen of Wöhler. The composition of this body has not been ascertained. It has been ascertained, however, that the substance from which it is obtained by the action of chlorhydric acid, is a silicide of magnesium, represented by the formula Mg_2Si , whence the formula of siliciuretted hydrogen is assumed to be H_4Si , analogous to that of marsh gas H_4C , a conclusion strongly confirmed by the composition of chloride of silicon, which is undoubtedly Cl_4Si , that is, a chloro-representative of siliciuretted hydrogen. Each primary hydride, hitherto considered, has yielded a remarkably stable acid, formed by the addition of four atoms of oxygen to the hydride; and hydride of silicon ought to behave in the same manner, thus:

Chlorhydric acid	H Cl	H ClO_4	Perchloric acid.
Sulphydric acid	$\text{H}_2 \text{S}$	$\text{H}_2 \text{S O}_4$	Sulphuric acid.
Phosphamine	$\text{H}_3 \text{P}$	$\text{H}_3 \text{P O}_4$	Phosphoric acid.
Hydride of Silicon	$\text{H}_4 \text{Si}$	$\text{H}_4 \text{Si O}_4$	Silicic acid.

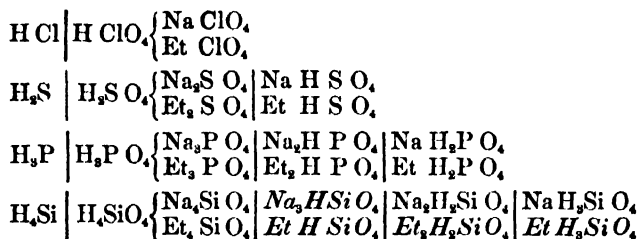
Now whether or not H_4SiO_4 is the correct formula for silicic acid, it is certain that the great majority of simple and well-defined silicates may be referred to that type, as illustrated in the table.

ORTHOSILICATES.

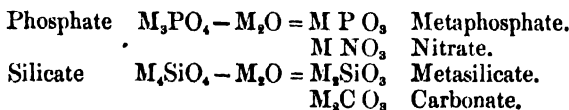
$\text{Et}_4 \text{SiO}_4$	Silicic ether.	$\text{Gl}_4 \text{SiO}_4$	Phenakite.
$\text{Li}_4 \text{SiO}_4$	Silicate of lithium.	$\text{Ce}_4 \text{SiO}_4$	Cerite.
$\text{Na}_4 \text{H}_4 \text{SiO}_4$	Silicate of sodium.	$\text{Fe}_4 \text{SiO}_4$	Fayelite.
$\text{Ca}_4 \text{SiO}_4$	Silicate of calcium.	Fe, Mn, SiO_4	Knebelite.
$\text{Mg}_4 \text{SiO}_4$	Olivine, Chrysolite.	$\text{Cu}_2 \text{H}_2 \text{SiO}_4$	Dioptase.
$\text{Ca}_2 \text{Mg}_2 \text{SiO}_4$	Batrachite.	$\text{Al}_2 \text{Ca SiO}_4$	Anorthite.
$\text{Zn}_4 \text{SiO}_4$	Zinc glance.	$\text{Al}_2 \text{Mn SiO}_4$	Karpholite.

This next table illustrates the general relations of the perchloric salts and ethers, to their sulphuric, phosphoric, and silicic analogues. The existence of the silicated compounds corresponding to the formulæ in italics, has not yet been established.

ACIDS, SALTS, AND ETHERS.

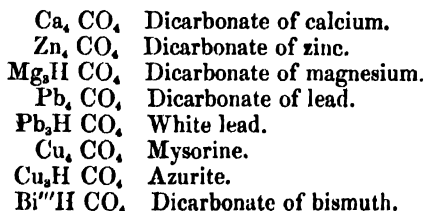


Considering the relations of ammonia and phosphuretted hydrogen, H_3N and H_3P respectively, and the relations of marsh gas, and siliuretted hydrogen, H_4C and H_4Si respectively, there should exist nitrates and carbonates having the general formulæ M_3NO_4 and M_4CO_4 respectively, corresponding to ordinary phosphates and silicates having the general formulæ M_3PO_4 and M_4SiO_4 respectively. It is observable, however, that in addition to ordinary phosphates and silicates, there are other phosphates and silicates, known respectively as metaphosphates and metasilicates, which differ from the ordinary salts by the loss of an atom of base, and that it is these metasalts to which ordinary nitrates and carbonates correspond, thus:

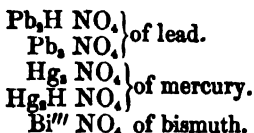


But chemists are acquainted with a considerable number of carbonates and nitrates, which may be called orthocarbonates and orthonitrates respectively, that do correspond in their formulæ with ordinary silicates and phosphates, as shown in the table.

ORTHOCARBONATES.



ORTHONITRATES.



The succeeding tables present lists of the principal ter-oxygen and tetra-oxygen mineral acids. Some of these acids are known only through the medium of their metal- and ethyl-representatives.

TER-OXYGEN ACIDS.

Chloric . . .	H Cl O ₄
Bromic . . .	H Br O ₄
Iodic . . .	H I O ₄
Nitric . . .	H N O ₄
Metaphosphoric	H P O ₄
Sulphurous . .	H ₂ S O ₄
Selenious . . .	H ₂ Se O ₄
Tellurous . . .	H ₂ T O ₄
Carbonic . . .	H ₂ C O ₄
Metasilicic . .	H ₂ Si O ₄
Titanic . . .	H ₂ Ti O ₄
Stannic . . .	H ₂ Sn O ₄
Vanadous . . .	H ₂ V ₂ O ₄
Phosphorous .	H ₂ P O ₄
Arsenious . . .	H ₂ As O ₄
Antimonous . .	H ₂ Sb O ₄
Bismuthous . .	H ₂ Bi O ₄
Boracic . . .	H ₂ B O ₄
Aluminous . .	H ₂ Al ₂ O ₄

TETRA-OXYGEN ACIDS.

Perchloric . .	H Cl O ₄
Periodic . . .	H I O ₄
Permanganic . .	H Mn ₂ O ₄
Sulphuric . . .	H ₂ S O ₄
Selenic	H ₂ Se O ₄
Telluric	H ₂ T O ₄
Oxalic	H ₂ C ₂ O ₄
Molybdic	H ₂ Mo ₂ O ₄
Vanadic	H ₂ V ₂ O ₄
Tungstic	H ₂ W ₂ O ₄
Chromic	H ₂ Cr ₂ O ₄
Manganic	H ₂ Mn ₂ O ₄
Ferric	H ₂ Fe ₂ O ₄
Orthonitric . . .	H ₂ N O ₄
Phosphoric . . .	H ₂ P O ₄
Arsenic	H ₂ As O ₄
Antimonic	H ₂ Sb O ₄
Orthocarbonic .	H ₂ C O ₄
Silicic	H ₂ Si O ₄

Hence the formula $\text{H}_x \text{R}_y \text{O}_z$, will represent the general type for an acid, where H_x represents the atoms of hydrogen, which, save in carbon compounds, are found to vary only from 1 to 4; where R_y represents the acid radicle, that is the chlorine, or sulphur, or phosphorus, or carbon, &c. which gives the special character to the acid, and which, save in carbon compounds, is usually confined to 1 or 2 elementary atoms; and where O_z represents the atoms of oxygen which generally range from 0 to 4, but occasionally extend to higher numbers.

[W. O.]

Friday, May 18, 1860.

CHARLES WHEATSTONE, Esq. F.R.S. Vice-President, in the Chair.

PROFESSOR WILLIAM THOMSON, F.R.S.

On Atmospheric Electricity.

STEPHEN GRAY, a pensioner of the Charter-house, after many years of enthusiastic and persevering devotion to electric science, closed his philosophical labours, about 130 years ago, with the following remarkable conjecture: "That there may be found a way to collect a greater quantity of the electrical fire, and consequently to increase the force of that power, which by several of these experiments, *si licet magna componere parvis*, seems to be of the same nature with that of thunder and lightning."

The inventions of the electrical machine and the Leyden phial immediately fulfilled these expectations as to collecting greater quantities of electric fire; and the surprise and delight which they elicited by their mimic lightnings and thunders, and above all by the terrible electric shock, had scarcely subsided when Franklin sent his kite messenger to the clouds, and demonstrated that the imagination had been a true guide to this great scientific discovery—the identity of the natural agent in the thunderstorm with the mysterious influence produced by the simple operation of rubbing a piece of amber, which two thousand years before had attracted the attention of those philosophers among the ancients who did not despise the small things of nature.

The investigation of atmospheric electricity immediately became a very popular branch of natural science; and the discovery of remarkable and most interesting phenomena quickly rewarded its cultivators. The foundation of all we now know was completed by Beccaria, in his observations on "the mild electricity of serene weather," nearly a hundred years ago. It was not until comparatively recent years that definite quantitative comparisons from time to time of the electric quality manifested by the atmosphere in one locality were first

obtained by the application of Peltier's mode of observation with his metrical electroscope. The much more accurate electrometer, and the greatly improved mode of observation invented by Delmann, have given for the electric intensity, at any instant, still more precise results; but have left something to desire in point of simplicity and convenience for general use, and have not afforded any means for continuous observation, or for the introduction of self-recording apparatus. The speaker had attempted to supply some of these wants, and he explained the construction and use of instruments which he had planned for this purpose, which were now exhibited to the meeting.

Apparatus for the observation of atmospheric electricity has essentially two functions to perform; to electrify a body with some of the natural electricity or with electricity produced by its influence; and to measure the electrification thus obtained.

The measuring apparatus exhibited consisted of three electrometers, which were referred to under the designations of (I.) The divided ring reflecting electrometer. (II.) The common house electrometer; and (III.) The portable electrometer.

(I.) The divided ring reflecting electrometer consists of:

(1) A ring of metal divided into two equal parts of which one is insulated, and the other connected with the metal case (5) of the instrument.

(2) A very light needle of sheet aluminium hung by a fine glass fibre, and counterpoised so as to make it project only to one side of this axis of suspension.

(3) A Leyden phial, consisting of an open glass jar, coated outside and inside in the usual manner, with the exception that the tinfoil of the inner coating does not extend to the bottom of the jar, which is occupied instead by a small quantity of sulphuric acid.

(4) A stiff straight wire rigidly attached to the aluminium needle, as nearly as may be in the line of the suspending fibre, bearing a light platinum wire linked to its lower end, and hanging down so as to dip into the sulphuric acid.

(5) A case protecting the needle from currents of air, and from irregular electric actions, and maintaining an artificially dried atmosphere round the glass pillar or pillars supporting the insulated half ring and the uncoated portion of the glass of the phial.

(6) A light stiff metallic electrode projecting from the insulated half ring through the middle of a small aperture in the metal case, to the outside.

(7) A wide metal tube of somewhat less diameter than the Leyden jar, attached to a metal ring borne by its inside coating, and standing up vertically to a few inches above the level of the mouth of the jar.

(8) A stiff wire projecting horizontally from this metal tube above the edge of the Leyden jar, and out through a wide hole in the case of the instrument to a convenient position for applying electricity to charge the jar with.

(9) A very light glass mirror, about three-quarters of an inch diameter, attached by its back to the wire (4), and therefore rigidly connected with the aluminium needle.

(10) A circular aperture in the case shut by a convex lens, and a long horizontal slit shut by plate glass, with its centre immediately above or below that of the lens, one of them above, and the other equally below the level of the centre of the mirror.

(11) A large aperture in the wide metal tube (7), on a level with the mirror (9), to allow light from a lamp outside the case, entering through the lens, to fall upon the mirror, and be reflected out through the plate glass window; and three or four fine metal wires stretched across this aperture to screen the mirror from irregular electric influences, without sensibly diminishing the amount of light falling on and reflected off it.

The divided ring (1) is cut out of thick strong sheet metal (generally brass). Its outer diameter is about 4 inches, its inner diameter $2\frac{1}{2}$; and it is divided into two equal parts by cutting it along a diameter with a saw. The two halves are fixed horizontally; one of them on a firm metal support, and the other on glass, so as to retain as nearly as may be their original relative position, with just the saw cut, from $\frac{1}{10}$ to $\frac{1}{20}$ of an inch broad, vacant between them. They are placed with their common centre as nearly as may be in the axis of the case (5), which is cylindrical, and placed vertically. The Leyden jar (3), and the tube (7), carried by its inside coating, have their common axis fixed to coincide as nearly as may be with that of the case and divided ring. The glass fibre hangs down from above in the direction of this axis, and supports the needle about an inch above the level of the divided ring. The stiff wire (4), attached to the needle, hangs down as nearly as may be along the axis of the tube (7).

Before using the instrument, the Leyden phial (3) is charged by means of its projecting electrode (8). When an electrical machine is not available, this is very easily done by the aid of a stick of vulcanite, rubbed by a piece of chamois leather. The potential of the charge thus communicated to the phial, is to be kept as nearly constant as is required for the accuracy of the investigation for which the instrument is used. Two or three rubs of the stick of vulcanite once a day, or twice a day, are sufficient when the phial is of good glass, well kept dry. The most convenient test for the charge of the phial is a proper electrometer or electroscope, of any convenient kind, kept constantly in communication with the charging electrode (8).

The electrometer (II.) is to be ordinarily used for that purpose in the Kew apparatus. Failing any such gauge electrometer or electroscope, a zinc-copper-water battery of ten, twenty, or more small cells, may be very conveniently used to test directly the sensibility of the reflecting electrometer, which is to be brought to its proper degree by charging its Leyden phial as much as is required.

In the use of this electrometer, the two bodies of which the difference of potentials is to be tested are connected, one of them, which is gene-

rally the earth, with the metal case of the instrument, and the other with the insulated half ring. The needle being, let us suppose, negatively electrified, will move towards or from the insulated half ring, according as the potential of the conductor connected with this half ring differs positively or negatively from that of the other conductor (earth) connected with the case. The mirror turns accordingly in one direction or the other through a small angle from its zero position, and produces a corresponding motion in the image of the lamp on the screen on which it is thrown.

I. The common house electrometer.—This instrument consists of:

(1) A thin flint-glass bell, coated outside and inside like a Leyden phial, with the exception of the bottom inside, which contains a little sulphuric acid.

(2) A cylindrical metal case, enclosing the glass jar, cemented to it round its mouth outside, extending upwards about an inch and a half above the mouth, and downwards to a metal base supporting the whole instrument, and protecting the glass against the danger of breakage.

(3) A cover of plate glass, with a metal rim, closing the top of the cylindrical case of the instrument.

(4) A torsion head, after the manner of Coulomb's balance, supported in the centre of the glass cover, and bearing a glass fibre which hangs down through an aperture in its centre.

(5) A light aluminium needle attached across the lower end of the fibre (which is somewhat above the centre of the glass bell), and a stiff platinum wire attached to it at right angles, and hanging down to near the bottom of the jar.

(6) A very light platinum wire, long enough to hang within one-eighth of an inch or so of the bottom of the jar, and to dip in the sulphuric acid.

(7) A metal ring, attached to the inner coating of the jar, bearing two plates in proper positions for repelling the two ends of the aluminium needle when similarly electrified, and proper stops to limit the angular motion of the needle to within about 45° from these plates.

(8) A cage of fine brass wire, stretched on brass framework, supported from the main case above by two glass pillars, and partially enclosing the two ends of the needle, and the repelling plates, from all of which it is separated by clear spaces, of nowhere less than one-fourth of an inch of air.

(9) A charging electrode, attached to the ring (7), and projecting over the mouth of the jar to the outside of the metal case (2), through a wide aperture, which is commonly kept closed by a metal cap, leaving at least one-quarter of an inch of air round the projecting end of the electrode.

(10) An electrode attached to the cage (8), and projecting over the mouth of the jar to the outside of the metal case (2), through the centre of an aperture, about a quarter of an inch diameter.

This instrument is adapted to measure differences of potential

between two conducting systems, namely, as one, the aluminium needle (5), the repelling plates (7), and the inner coating of the jar, and, as the other, the insulated cage (8). This latter is commonly connected by means of its projecting electrode (10), with the conductor to be tested. The two conducting systems, if through their projecting electrodes connected by a metallic wire, may be electrified to any degree, without causing the slightest sensible motion in the needle. If, on the other hand, the two electrodes of these two systems are connected with two conductors, electrified to different potentials, the needle moves away from the repelling plates; and if by turning the torsion head it is brought back to one accurately marked position, the number of degrees of torsion required is proportional to the square of the difference of potentials thus tested.

In the ordinary use of the instrument, the inner coating of the Leyden jar is charged negatively, by an external application of electricity through its projecting electrode (9). The degree of the charge thus communicated, is determined by putting the cage in connexion with the earth through its electrode (10), and bringing the needle by torsion to its marked position. The square root of the number of degrees of torsion required to effect this, measures the potential of the Leyden charge. This result is called the reduced earth reading. When the atmosphere inside the jar is kept sufficiently dry,—this charge is retained from day to day with little loss; not more, often, than 1 per cent. in the 24 hours.

In using the instrument the charging electrode (9) of the jar is left untouched, with the aperture through which it projects closed over it by the metal cap referred to above. The electrode (10) of the cage, when an observation is to be made, is connected with the conductor to be tested, and the needle is brought by torsion to its marked position. The square root of the number of degrees of torsion now required measures the difference of potentials between the conductor tested and the interior coating of the Leyden jar. The excess, positive or negative, of this result above the reduced earth reading, measures the excess of the potential, positive or negative, of the conductor tested above that of the earth; or simply the potential of the conductor tested, if we regard that of the earth as zero.

III. The portable electrometer is constructed on the same electrical principles as the house electrometer just described. The mode of suspension of the needle is however essentially different; and a varied plan of connection between the different electrical parts has been consequently adopted as more convenient. In the portable electrometer, the needle is firmly attached at right angles to the middle of a fine platinum wire, tightly stretched in the axis of a brass tube with apertures in its middle to allow the needle to project on the two sides. One end of the platinum wire is rigidly connected with this tube; the other is attached to a graduated torsion head. The brass tube carries two metal plates in suitable positions to repel the two

ends of the needle in contrary directions, and metal stops to limit its angular motion within a convenient range. The conducting system composed of these different parts is supported from the metal cover or roof of the jar, by three glass stems. The torsion head is carried round by means of a stout glass bar, projecting down from a pinion centered on the lower side of this cover, and turned by the action of a tangent screw presenting a milled head, to the hand of the operator outside. The conducting system thus borne by insulating supports is connected with the outside conductor to be tested by means of an electrode passing out through the centre of the top of the case by a wide aperture in the centre of the pinion. A wire cage, surrounding the central part of the tube and the needle and repelling plates, is rigidly attached to the interior coating of the Leyden jar. It carries two metal sectors, or "bulkheads," in suitable positions to attract the two ends of the needle, which however is prevented from touching them by the limiting stops referred to above. The effect of these attracting plates, as they will be called, is to increase very much the sensibility of the instrument. The square root of the number of degrees of torsion required to bring the needle to a sighted position near the repelling plates, measures the difference of potentials between the cage and the conducting system, consisting of tube torsion-head repelling plates and needle. The metal roof of the jar is attached to a strong metal case, cemented round the outside of the top of the jar, and enclosing it all round and below, to protect it from breakage when being carried about. There are sufficient apertures in this case, opened by means of a sliding piece, to allow the observer to see the needle and graduated circle (torsion head), when using the instrument. On the outside of the roof of the jar a stout glass stem is attached, which supports a light stiff metallic conductor, by means of which a burning match is supported, at the height of two or three feet above the observer. This conductor is connected by means of a fine wire with the electrometer, in the manner described above, through the centre of the aperture in the roof. An artificially dried atmosphere is maintained around this glass stem, by means of a metal case surrounding it, and containing receptacles of gutta percha, or lead, holding suitably shaped pieces of pumice-stone moistened with sulphuric acid. The conductor which bears the match projects upwards through the centre of a sufficiently wide aperture, and bears a small umbrella, which both stops rain from falling into this aperture, and diminishes the circulation of air, owing to wind blowing round the instrument, from taking place to so great a degree as to do away with the dryness of the interior atmosphere required to allow the glass stem to insulate sufficiently. The instrument may be held by the observer in his hand in the open air without the assistance of any fixed stand. A sling attached to the instrument and passing over his left shoulder, much facilitates operations, and renders it easy to carry the apparatus to the place of observation, even if up a rugged hill side, with little risk of accident.

The burning match in the apparatus which has just been described, performs the collecting function referred to above. The collector employed for the station apparatus, whether the reflecting electrometer or the common house electrometer is used, is an insulated vessel of water, allowed to flow out in a fine stream through a small aperture at the end of a pipe projecting to a distance of several feet from the wall of the building in which the observations are made.

The principle of collecting, whether by fire or by water, in the observation of atmospheric electricity, was explained by the speaker thus:—The earth's surface is, except at instants, always found electrified, in general negatively, but sometimes positively.

"If a large sheet of metal were laid on the earth in a perfectly level district, and if a circular area of the same metal were laid upon this sheet, and after the manner of Coulomb's proof plane, were lifted by an insulated handle and removed to an electrometer within doors, a measure of the earth's electrification, at the time, would be obtained; or, if a ball, placed on the top of a conducting rod in the open air, were lifted from that position by an insulating support, and carried to an electrometer within doors, we should also have, on precisely the same principle, a measure of the earth's electrification at the time. If the height of the ball in this second plan were equal to one-sixteenth of the circumference of the disc used in the first plan, the electrometric indications would be the same, provided the diameter of the ball is small, in comparison with the height to which it is raised in the air, and the electrostatic capacity of the electrometer is small enough not to take any considerable proportion of the electricity from the ball in its application. The idea of experimenting by means of a disc laid flat on the earth, is merely suggested for the sake of illustration, and would obviously be most inconvenient in practice. On the other hand, the method by a carrier ball, instead of a proof plane, is precisely the method by which, on a small scale, Faraday investigated the distribution of electricity induced on the earth's surface, by a piece of rubbed shellac; and the same method, applied on a suitable scale for testing the natural electrification of the earth in the open air, has given in the hands of Delmann, of Creuznach, the most accurate results hitherto published in the way of electro-meteorological observation. If, now, we conceive an elevated conductor first belonging to the earth, to become insulated and to be made to throw off and to continue throwing off portions from an exposed position of its own surface, this part of its surface will quickly be reduced to a state of no electrification, and the whole conductor will be brought to such a potential as will allow it to remain in electrical equilibrium in the air with that portion of its surface neutral. In other words, the potential throughout the insulated conductor is brought to be the same as that of the particular equi-potential surface in the air, which passes through the point of it from which matter breaks away. A flame, or the heated gas passing from a burning match, does precisely this: the flame itself, or the highly heated gas close to the match, being a conductor which is

constantly extending out and gradually becoming a non-conductor. The drops into which the jet issuing from the insulated conductor breaks, on the plan introduced by the writer, produce the same effects with more pointed decision, and with more of dynamical energy to remove the rejected matter with the electricity which it carries from the neighbourhood of the fixed conductor."—*Nichol's Cyclopædia*, 2nd edition, article "*Electricity, Atmospheric*."

After having given so much of these explanations as seemed necessary to convey a general idea of the principles on which the construction of the instruments of investigation depended, the speaker proceeded to call attention to the special subject proposed for consideration this evening.

What is terrestrial atmospheric electricity? Is it electricity of earth, or electricity of air, or electricity of watery or other particles in the air? An endeavour to answer these questions was all that was offered; abstinence from speculation as to the origin of this electric condition of our atmosphere, and its physical relations with earth, air, and water, having been painfully learned by repeated and varied failure in every attempt to see beyond facts of observation. In serene weather, the earth's surface is generally, in most localities hitherto examined, found negatively or resinously electrified; and when this fact alone is known it might be supposed that the globe is merely electrified as a whole with a resinous charge, and left insulated in space.

But it is to be remarked, that the earth, although insulated in its atmospheric envelope, being in fact a conductor touched only by air, one of the best although not the strongest of insulators, cannot with its atmosphere be supposed to be insulated, so as to hold an electric charge in interplanetary space. It has been supposed, indeed, that outside the earth's recognised atmosphere there exists something or nothing in space which constitutes a perfect insulator; but this supposition seems to have no other foundation than a strange idea that electric conductivity is a strength or a power of matter rather than a mere *non-resistance*. In reality we know that air highly rarefied by the air-pump, or by other processes, as in the construction of the "vacuum tubes," by which such admirable phenomena of electric light have recently been seen in this place, becomes extremely weak in its resistance to the transference of electricity through it, and begins to appear rather as a conductor than an insulator. One hundred miles or upwards from the earth's surface, the air in space cannot in all probability have resisting power enough to bear any such electric forces, as those which we generally find even in serene weather in the lower strata. Hence, we cannot, with Peltier, regard the earth as a resinously charged conductor, insulated in space, and subject only to accidental influences from temporary electric deposits in clouds, or air round it; but we must suppose that there is always *essentially* in the higher aerial regions a distribution arising from the self-relief of the outer highly rarefied air by disruptive discharge. This electric stratum must constitute very nearly the electro-polar complement to all the electricity that exists on

the earth's surface, and in the lower strata of the atmosphere; in other words, the total quantity of electricity, reckoned as excess of positive above negative, or of negative above positive, in any large portion of the atmosphere, and on the portion of the earth's surface below it, must be very nearly zero. The quality of non-resistance to electric force of the thin interplanetary air being duly considered, we might regard the earth, its atmosphere, and the surrounding medium as constituting respectively the inner coating, the di-electric (as it were glass), and the outer coating of a great Leyden phial, charged negatively; and even if we were to neglect the consideration of possible deposits of electricity through the body of the di-electric itself, we should arrive at a correct view of the electric indications discoverable at any one time and place of the earth's surface. In fact, any kind of "collector," or plan for collecting electricity from or in virtue of the natural "terrestrial atmospheric electricity," gives an effect simply proportional to the electrification of the earth's surface then and there. The methods of collecting by fire and water which the speaker exhibited, gave definitively, in the language of the mathematical theory, the "electric potential" of the air at the point occupied by the burning end of the match or by the portion of the stream of water where it breaks into drops. If the apparatus is used in an open plane, and care be taken to eliminate all disturbance due to the presence of the electrometer itself and of the observer above the ground, the indicated effect, if expressed in absolute electrostatic measure, and divided by the height of the point tested above the ground, has only to be, (according to an old theorem of Coulomb's, corrected by Laplace,) divided by four times the ratio of the circumference of a circle to its diameter, to reduce it to an expression of the number of units in absolute electrostatic measure of the electricity per unit of area of the earth's surface at the time and place. The mathematical theory does away with every difficulty in explaining the various and seemingly irreconcilable views which different writers have expressed, and explanations which different observers have given of the functions of their testing apparatus. In the present state of electric science, the most convenient and generally intelligible way to state the result of an observation of terrestrial atmospheric-electricity, in absolute measure, is in terms of the number of elements of a constant galvanic battery, required to produce the same difference of potentials as exists between the earth and a point in the air at a stated height above an open level plane of ground. Observations with the portable electrometer had given in ordinary fair weather, in the island of Arran, on a flat open sea beach, readings varying from 200 to 400 Daniel's elements, as the difference of potentials between the earth and the match, at a height of 9 feet above it. Hence, the intensity of electric force perpendicular to the earth's surface, must have amounted to from .22 to .44 Daniel's elements, per foot of air. In fair weather, with breezes from the east or north-east, he had often found from 6 to 10 times the higher of these intensities.

Even in fair weather, the intensity of the electric force in the air near the earth's surface is perpetually fluctuating. The speaker had often observed it, especially during calms or very light breezes from the east, varying from 40 Daniel's elements per foot, to three or four times that amount during a few minutes; and returning again as rapidly to the lower amount. More frequently he had observed variations from about 30 to about 40, and back again, recurring in uncertain periods of perhaps about two minutes. These gradual variations cannot but be produced by electrified masses of air or cloud, floating by the locality of observation. Again, it is well known that during storms of rain, hail, or snow, there are great and sometimes sudden variations of electric force in the air close to the earth. These are undoubtedly produced, partly as those of fair weather, by motions of electrified masses of air and cloud; partly by the fall of vitreously or resinously electrified rain, leaving a corresponding deficiency in the air or cloud from which it falls; and partly by disruptive discharges (flashes of lightning) between masses of air or cloud, or between either and the earth. The consideration of these various phenomena suggested the following questions, and modes of observation for answering them.

Question 1. How is electricity distributed through the different strata of the atmosphere to a height of five or six miles above the earth's surface in ordinary fair weather? To be answered by electrical observations in balloons at all heights up to the highest limit, and simultaneous observations at the earth's surface.

Q. 2. Does electrification of air close to the earth's surface, or within a few hundred feet of it, sensibly influence the observed electric force? and if so, how does it vary with the weather, and with the time of day or year? The first part of this question has been answered very decidedly in the affirmative, first, for large masses of air within a few hundred yards of the earth's surface, by means of observations made simultaneously at a station near the seashore in the island of Arran, and at one or other of several stations at different distances, within six miles of it, on the sides and summit of Goatfell. After that it was found, by simultaneous observations made at a window in the Natural Philosophy Lecture Room, and on the College Tower of the University of Glasgow, that the influence of the air within 100 feet of the earth's surface was always sensible at both stations, and often paramount at the lower. Thus, for example, when in broken weather, the superficial electrification of the outside of the lecture room, about 20 feet above the ground, in a quadrangle of buildings, was found positive, the superficial electrification of the sides of the tower, about 70 feet higher, was often found negative or nearly zero; and this sometimes even when the positive electrification of the sides of the building at the lower station equalled in amount an ordinary fair weather negative. This state of things could only exist in virtue of a negative electrification of the circumambient air, inducing a positive electrification on the ground and sides of the quadrangle, but not sufficient to counter-

balance the influence on the higher parts of the tower of more distant positively electrified aerial masses.

A long continuation of such systems of simultaneous observation—not in a town only, but in various situations of flat and of mountainous country, on the sea coast as well as far inland, in various regions of the world—will be required to obtain the information asked for in the second part of this question.

Q. 3. Do the particles of rain, hail, and snow in falling through the air possess absolute charges of electricity? and if so, whether positive or negative, and of what amounts in different conditions as to place and weather? Attempts to answer this question have been made by various observers, but as yet without success; as for instance by an “electro-pluviometer,” tried at Kew many years ago. By using a sufficiently well insulated vessel to collect the falling particles, it is quite certain that a decided answer may be obtained with ease for the cases of hail and snow. Inductive effects produced by drops splashing away from the collecting vessel, if exposed to the electric force of the air in an open position, or inductive effects of the opposite kind produced by drops splashing away from surrounding walls or screens and falling into the collecting vessel, if not in an exposed position, make it less easy to ascertain the electrical quality of rain; but, by taking means to obviate the disturbing effects of these influences, the speaker hoped to arrive at definite results.

It would have been more satisfactory to have been able to conclude a discourse on atmospheric electricity otherwise than in questions, but no other form of conclusion would have been at all consistent with the present state of knowledge.

The discourse was illustrated by the use of the mirror electrometer reflecting a beam of light from the electric lamp, and throwing it on a white screen, where its motions were measured by a divided scale. The principle of the water-dropping collector was illustrated by allowing a jet of water to flow by a fine nozzle into the middle of the lecture-room, from an uninsulated metal vessel of water and compressed air, and collecting the drops in an insulated vessel on the floor. This vessel was connected with the testing electrode of the reflecting electrometer; and it was then found to experience a continually increasing negative electrification, when fixed positively electrified bodies were in the neighbourhood of the nozzle. If the same experiment were made in ordinary fair weather in the open air, instead of under the roof and within the walls of the lecture-room, the same result would be observed, without the presence of any artificially electrified body. The vessel from which the water was discharged was next insulated; and other circumstances remaining unvaried, it was shown that this vessel became rapidly electrified to a certain degree of positive potential, and the falling drops ceased to communicate any more electricity to the vessel in which they were gathered.

The influence of electrified masses of air was illustrated by carrying about the portable electrometer, with its match burning, to different

parts of the lecture-room, while insulated spirit lamps connected with the positive and negative conductor of an electrical machine, burned on the two sides. The speaker observed the indications on the portable electrometer; but the potentials thus measured, were seen by the audience marked on the scale by the spot of light; the reflecting electrometer being kept connected with the portable electrometer in all its positions, by means of a long fine wire. It was found that when the burning match was on one side of a certain surface dividing the air of the lecture-room, the potential indicated was positive, and on the other side negative.

The water-dropping collector constructed for the self-registering apparatus to be used at Kew, had been previously set upon the roof of the Royal Institution, and an insulated wire (Beccaria's "Deferent Wire") led down to the reflecting electrometer on the lecture-room table. The electric force in the air above the roof was thus tested several times during the meeting; and it was at first found to be, as it had been during several days preceding, somewhat feeble positive (corresponding to a feeble negative electrification of the earth's surface, or rather housetops, in the neighbourhood). This was a notunfrequent electrical condition of days, such as these had been of dull rain, with occasional intervals of heavier rain and of cessation. The natural electricity was again observed by means of the reflecting electrometer during several minutes near the end of the discourse; and was found instead of the weak positive which had been previously observed, to be strong positive of three or four times the amount. Upon this the speaker quoted* an answer, which Prior Ceca had given to a question Beccaria had put to him "concerning the state of electricity when the weather clears up." "If, when the rain has ceased (the prior said to me) a strong excessive† electricity obtains, it is a sign that the weather will continue fair for several days; if the electricity is but small, it is a sign that such weather will not last so much as that whole day, and that it will soon be cloudy again, or even will again rain." The climate of this country is very different from that of Piedmont, where Beccaria and his friend made their observations, but their rule as to the "electricity of clearing weather," has been found frequently confirmed by the speaker. He therefore considered, that although it was still raining at the commencement of the meeting, the electrical indications they had seen gave fair promise‡ for the remainder of this evening, if not for a longer period. There can be no doubt but that electric indications, when sufficiently studied, will be found important additions to our means for prognosticating the weather;

* From Beccaria's first letter "On Terrestrial Atmospheric Electricity during Serene Weather."—*Garzogna di Mondovì*, May 16. 1775.

† i.e. vitreous, or positive.

‡ At the conclusion of the meeting it was found that the rain had actually ceased. The weather continued fair during the remainder of the night, and three or four of the finest days of the season followed.

and the speaker hoped soon to see the atmospheric electrometer generally adopted as a useful and convenient weather-glass.

The speaker could not conclude without guarding himself against any imputation of having assumed the existence of two electric fluids or substances, because he had frequently spoken of the vitreous and resinous electricities. Dufay's very important discovery of two modes or qualities of electrification, led his followers too readily to admit his supposition of two distinct electric fluids. Franklin, Æpinus, and Cavendish, with a hypothesis of one electric fluid, opened the way for a juster appreciation of the *unity* of nature in electric phenomena. Beccaria, with his "electric atmospheres," somewhat vaguely struggled to see deeper into the working of electric force, but his views found little acceptance, and scarcely suggested inquiry or even meditation. The 18th century made a school of science for itself in which, for the not unnatural dogma of the earlier schoolmen "matter cannot act where it is not," was substituted the most fantastic of paradoxes, *contact does not exist*. Boscovich's theory was the consummation of the 18th century school of physical science. This strange idea took deep root, and from it grew up a barren tree, exhausting the soil and overshadowing the whole field of molecular investigation, on which so much unavailing labour was spent by the great mathematicians of the early part of our 19th century. If Boscovich's theory no longer cumber the ground, it is because one true philosopher required more light for tracing lines of electric force.

Mr. Faraday's investigation of electrostatic induction influences now every department of physical speculation, and constitutes an era in science. If we can no longer regard electric and magnetic fluids attracting or repelling at a distance as realities, we may now also contemplate as a thing of the past that belief in atoms and in vacuum, against which Leibnitz so earnestly contended in his memorable correspondence with Dr. Samuel Clarke.

We now look on space as full. We know that light is propagated, like sound, through pressure and motion. We know that there is no substance of caloric—that inscrutably minute motions cause the expansion which the thermometer marks, and stimulate our sensation of heat—that fire is not laid up in coal more than in this Leyden phial, or this weight: there is potential fire in each. If electric force depends on a residual *surface action*, a resultant of an inner tension experienced by the insulating medium, we can conceive that electricity itself is to be understood as not an accident, but an essence of matter. Whatever electricity is, it seems quite certain that electricity in motion is *heat*; and that a certain alignment of axes of revolution in this motion is *magnetism*. Faraday's magneto-optic experiment makes this not a hypothesis, but a demonstrated conclusion. Thus a rifle bullet keeps its point foremost; Foucault's gyroscope finds the earth's axis of palpable rotation; and the magnetic needle shows that more subtle rotatory movement in matter of the earth, which we call terrestrial magnetism, all by one and the same dynamical action.

It is often asked, are we to fall back on facts and phenomena, and give up all idea of penetrating that mystery which hangs round the ultimate nature of matter? This is a question that must be answered by the metaphysician, and it does not belong to the domain of Natural Philosophy. But it does seem that the marvellous train of discovery, unparalleled in the history of experimental science, which the last years of the world has seen to emanate from experiments within these walls, must lead to a stage of knowledge, in which laws of inorganic nature will be understood in this sense—that one will be known as essentially connected with all, and in which unity of plan through an inexhaustibly varied execution, will be recognized as a universally manifested result of creative wisdom.

[W. T.]

Friday, January 18, 1861.

WILLIAM ROBERT GROVE, Esq. Q.C. F.R.S. Vice-President,
in the Chair.

JOHN TYNDALL, Esq. F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION.

On the Action of Gases and Vapours on Radiant Heat.

THE discourse commenced by a reference to the researches of Leslie, Forbes, and Knoblauch; but more especially to the admirable investigations of Melloni on Radiant Heat. These eminent men had left the gaseous form of matter practically untouched, and to extend our knowledge into this wide region was the object of the investigation on which the present discourse was founded.

The apparatus made use of, and which was applied in the experiments of the evening, consists of the following parts:—

1. A copper cube C, containing water kept constantly boiling, and one of whose faces, coated with lamp-black, forms the source of radiant heat.

2. A brass tube, 2·4 inches in diameter, which is divided into two portions, α and β .

α . The portion of the tube intended to receive the gases and vapours; it is stopped air-tight at its two ends by plates of rock-salt, and is attached to a good air-pump, by which it can be exhausted at pleasure. The length is 4 feet.

β . An air-tight chamber between the tube α and the cube C. It is kept constantly exhausted, and the calorific rays therefore pass from the radiating plate through a vacuum into the tube, thus retaining the quality which belonged to them at the moment of emission.

To prevent the transmission of heat by conduction from the cube C to the tube α , the chamber β is partly embraced by an annular space, in which cold water continually circulates.

3. A thermo-electric pile furnished with two conical reflectors, and connected with an excellent galvanometer. One of the faces of the pile receives the rays which have passed through the tube α .

4. A second copper cube C', also filled with boiling water, and whose rays fall upon the second face of the thermo-electric pile. The two cubes C and C', thus radiating upon the opposite faces of the pile, tend, of course, to neutralise each other.

Between the cube C' and the adjacent face of the pile a screen S is

introduced, being attached to an apparatus of Ruhmkorff's, capable of extremely fine motion ; by the partial advance or withdrawal of this screen the two sources of heat can be caused to neutralise each other perfectly.

The tube α and the chamber β , being both exhausted, the needle of the galvanometer is brought exactly to zero by means of the screen S. The gas or vapour to be experimented with is now admitted into the tube α , and if it possess any sensible absorbing power, it will destroy the previously existing equilibrium. The consequent deflection of the galvanometer, properly reduced, is the measure of the absorption. In this way the action of eight gases and thirteen vapours have been examined, and also the action of atmospheric air.

Oxygen, hydrogen, nitrogen, and atmospheric air, respectively absorb about 0.3 per cent. of the calorific rays ; this is the feeblest action which has been observed.

The most energetic action is that of olefiant gas, which at the tension of one atmosphere absorbs 81 per cent. of the calorific rays. Between those extremes stand carbonic oxide, carbonic acid, nitrous oxide, and sulphuretted hydrogen.

Below a certain tension, which varies for different gases, the amount of heat absorbed is exactly proportional to the density of the gas. Above this tension, the rays on which the principal absorptive energy is exerted, become gradually exhausted, so that every augmentation of density produces a diminished effect.

In the case of olefiant gas, for example, where a unit measure $\frac{1}{50}$ th of a cubic inch in capacity was made use of ; for a series of fifteen such measures, the absorption was exactly proportional to the quantity of gas ; subsequently, the ratios of the successive absorptions approached gradually to equality. The absorption produced by a single measure of olefiant gas of the above volume, moved the index of the galvanometer through an angle of 2.2 degrees ; the tension of the gas being only $\frac{1}{1000}$ th of an atmosphere.

In the case of vapours, the most energetic is that of sulphuric ether ; the least energetic is that of bisulphide of carbon. Comparing small volumes and equal tensions, the absorptive energy of sulphuric ether vapour is ten times that of olefiant gas, and ten thousand times that of oxygen, hydrogen, nitrogen, or atmospheric air.

On a fair November day the aqueous vapour in the atmosphere produced fifteen times the absorption of the true air itself. It is on rays emanating from a source of comparatively low temperature that this great absorptive energy is exerted ; hence the aqueous vapour of the atmosphere must act powerfully in intercepting terrestrial radiation ; its changes in quantity would produce corresponding changes of climate ; subsequent researches must decide whether this *vera causa* is competent to account for the climatal changes which geologic researches reveal.

Oxygen obtained from the electrolysis of water exerted four times the absorptive energy of the same substance when caused to pass

through iodide of potassium; the greater action being due to the presence of ozone.

The radiative power of gases was examined by causing them to pass over a heated sphere of metal, and ascend in a column in front of the thermo-electric pile; various precautions were taken to secure accuracy in the results. It was found that the order of radiation was exactly that of absorption; that any atom or molecule which is capable of accepting motion from agitated ether, is capable in precisely the same degree of imparting motion to still ether. Films of gas on surfaces of polished metal were found to act like coats of varnish.

The speaker also investigated the physical connection of radiation, absorption, and conduction. In the foregoing experiments *free* atoms and molecules were dealt with, and upon them individually was fixed the responsibility of the effects observed. These effects are thus detached from considerations of cohesion and aggregation, which suggest themselves in the case of liquids and solids.

The reciprocity of absorption and radiation is a simple mechanical consequence of the theory of an ether.

But why is one molecule competent to stop or generate a calorific flux so much more powerfully than another? The experiments prompt the following reply:—The elementary gases which have been examined all exhibit extremely feeble powers both of absorption and radiation, in comparison with the compound ones. In the former case we have oscillating atoms, in the latter oscillating systems of atoms. Uniting the atomic theory with the conception of an ether, it follows that the *compound* molecule which furnishes *points d'appui* to the ether must be capable of accepting and generating motion in a far greater degree than the single atom, which we may figure to our minds as an oscillating sphere. Thus oxygen and hydrogen, which, taken separately, or united mechanically, produce a scarcely sensible effect, when united chemically to form oscillating systems as in aqueous vapour, produce a powerful effect. Thus also, nitrogen and hydrogen, which when separate or mixed, produce but little action, when combined to form ammonia, produce a great action. So also nitrogen and oxygen, which when mixed as in air, are feeble absorbers and radiators, when united to oscillating systems, as in nitrous oxide, are very powerful in both capacities. Comparing small volumes and equal tensions, the action of nitrous oxide is 250 times that of air; a fact which perhaps furnishes a stronger presumption than any previously existing, that air is a *mixture*, and not a compound. Carbonic oxide is about 100 times as powerful as its constituent oxygen; carbonic acid is 150 times as powerful, while olefiant gas, as already remarked, is 1000 times as powerful as its constituent hydrogen. In the case of the hydro-carbon vapours, where the atomic groups attain a higher degree of complexity, the action is even greater than that of olefiant gas.

The speaker also referred to the experiments and observations of Niepce, Angstrom, and Foucault; but more especially to the admirable researches of Kirchhoff and Bunsen, as regards the influence of the

period of oscillation on the rate of absorption. He pointed out how the grouping of atoms to systems in a resisting medium must tend to make their periods of oscillation longer, and thus bring them into isochronism with the periods of the obscure radiations made use of in the experiments.

With regard to conduction, the speaker would illustrate his views by reference to two substances—rock-salt and alum. He was once surprised to observe the great length of time required by a heated mass of rock-salt to cool; but this was explained by the experiments of Mr. Balfour Stewart, who shows that rock-salt is an exceedingly feeble radiator. The meaning of this is that the molecules of the salt glide through the ether with small loss of *vis viva*. But the ease of motion which they are thus proved to enjoy must facilitate their mutual collision. The motion of the molecules, instead of being expended on the ether between them, and then communicated in part to the ether external to the mass, is transferred freely from particle to particle; or in other words, is freely conducted. This *a priori* conclusion is completely verified by the author's experiments, which prove rock-salt to be an excellent conductor. It is quite the reverse with alum. Mr. Balfour Stewart's experiments prove it to be an excellent radiator, and the author's experiments show it to be an extremely bad conductor. Thus it imparts with ease its motion to the ether, and for this very reason finds difficulty in transferring it from particle to particle; its molecules are in fact so constituted that when one of them approaches its neighbour, a swell is produced in the intervening ether; this motion is immediately communicated to the ether outside, and is thus lost for the purposes of conduction. The lateral waste prevents the motion from penetrating the alum to any great extent, and hence it is pronounced a bad conductor. These considerations seem to reduce the phenomena of absorption, radiation, and conduction to the simplest mechanical principles.

[J. T.]

Friday, February 22, 1861.

SIR RODERICK I. MURCHISON, D.C.L. F.R.S. Vice-President,
in the Chair.

PROFESSOR FARADAY, D.C.L. F.R.S.

On Platinum.

THE discourse was founded on the recent investigations of MM. Henri Ste-Claire Deville and H. Debray regarding the characters and conditions of the platiniferous metals; and the new process of working the ore which they have established on their results. Wherever platinum occurs, it is usually, if not always accompanied by five other remarkable metals; namely, Ruthenium, Osmium, Iridium, Rhodium, and Palladium: and in addition, by other substances, as iron, copper, gold, silver, and sand. Being washed, the heavy particles are left as the general ore of platinum; this metal constituting by far the largest part of the substances.

The six metals, when obtained apart and purified, form two groups of three each; each group having an equivalent number very different from that of the other group, as appears in this table:—

Equivalent number, 95·5.		Equivalent number, 53.	
1. Osmium . Spec. grav.	21·40	2. Ruthenium . Spec. grav.	11·3
3. Iridium „	21·15	4. Rhodium „	12·1
5. Platinum „	21·15	6. Palladium „	11·8

The three in the first group have the same equivalent number, and nearly the same specific gravity; but osmium takes the place of platinum as the heaviest of bodies. The equivalent number of the second group is alike for all, but it is little more than half that of the former group. The specific gravity also of the group is little more than half that of the former group: from which it results that an equivalent of any of these will have very nearly the same volume as an equivalent of any one of the heavier group.

There are certain analogies between 1 and 2; 3 and 4; 5 and 6; platinum is more like palladium than like the other metals. These numbers also represent the order of fusibility. Osmium has not as yet been fused; the rest have, in the order given. Platinum appears among them as a comparatively easily fusible metal. They are all volatile at very high temperatures, even osmium disappearing whilst the mass remains solid.

The platinum has usually been obtained from these ores (after they have been well washed, sifted, and mechanically separated) by the action of nitro-muriatic acid; which, bringing the platinum into solution, supplies a fluid which, on the addition of muriate of ammonia, &c., throws down a precipitate of ammonio-chloride of platinum. This, washed, dried, and heated, gives spongy metallic platinum; which being then pressed, heated, and hammered, yields massive platinum; the aggregation of the particles taking place entirely by adhesion and welding. Instead of forming a solution by acids, Deville proposes to employ a heat fluxion process; and instead of welding, to fuse the metal together at the last by intense heat, obtained by the use of the oxy-hydrogen or the oxy-coal-gas blowpipe. The ore, properly prepared, is mixed with its weight of galena, or native sulphuret of lead, and half its weight of metallic lead; it is then heated and well stirred together, the iron and some other metals are taken up by the sulphur of the galena, the platinum and other metals are taken possession of by the lead, and when the action is well effected, the access of air is adjusted until the remaining part of the sulphuret is decomposed, and only platiniferous lead left at the bottom part of the crucible or furnace, with scorix upon it. The former is separated, and then heated, exposed to air until much of the lead is oxidized; which, escaping as litharge, leaves at last an alloy of lead and platinum, containing not more than 10 or even 5 per cent. of lead. Such an alloy of platinum requires a very high temperature to fuse it, and this is therefore attained and applied in furnaces constructed of chalk-lime, heated by the insertion of gas blowpipes. The heat first melts the alloy, and being combined with oxygen in a little excess, the remaining lead is rapidly oxidized and dissipated in fumes, and then being raised and continued, any gold, copper, osmium, or other metals, except iridium and rhodium, are also converted into vapour and driven off. The platinum remaining is at last heated to a still higher degree, and is either cast into flat cakes or granulated; and this has been done with quantities weighing even as much as 40 lbs.

The resulting metal contains some iridium and some rhodium, being in fact an alloy of platinum; but it is an alloy which being harder than platinum, and even less liable than it to the chemical action of acids and other chemical agents, is as useful as the pure substance in the ordinary applications of the metal. As iridium and rhodium have no employment at present better than that of alloying platinum, their quantity has been purposely increased until it has made as much as 25 per cent. of the mass.

A mixed process has been devised by MM. Deville and Debray, which gives a platinum purer than any heretofore obtained. It is then as soft and ductile as silver. But for this process, for general directions and minute particulars, and for most interesting matter about all the metals of the platinum group, the reader is referred to Volumes LVI. and LXI. of the *Annales de la Chimie*.

[M. F.]

Friday, March 1, 1861.

SIR HENRY HOLLAND, Bart. M.D. F.R.S. in the Chair.

HENRY ENFIELD ROSCOE, Esq.

PROFESSOR OF CHEMISTRY IN OWEN'S COLLEGE, MANCHESTER.

On Bunsen and Kirchhoff's Spectrum Observations.

THE speaker commenced by stating that the researches of Bunsen and Kirchhoff, which he had the honour of bringing before his audience, marked a new era in the science of Analytical Chemistry; that by means of these discoveries the composition of terrestrial matter becomes revealed to us with a degree of accuracy and delicacy as yet unheard of, so that chemical elements supposed to be of rare and singular occurrence, are shown to be most commonly and widely distributed, and on the first practical application of this new method of analysis two new and hitherto undetected alkaline metals have been discovered.

The importance of these researches becomes still more strikingly apparent, when we hear that the conclusions derived from them outstep the bounds of our planet, enabling us to determine with all the certainty of definite experiment the actual presence of a number of elementary bodies in the sun.

The colours which certain bodies impart to flame, have long been used by chemists as a test for the presence of such bodies. Thus soda brought into a colourless flame produces a bright yellow light, and substances containing soda in any form give this yellow colour. Potash gives a violet flame, lithia and strontia impart to flame a crimson colour, whilst salts of barium tinge it green. These colours are produced by the incandescence or luminosity of the heated vapour of the various bodies placed in the flame. It is only because these substances are volatile, or become gases at the temperature of the flame, that we observe the peculiar colour. If any substance, such as platinum, which is not volatile at the temperature of the flame, be placed in it, no coloration is observed. The higher the temperature of the flame into which the same substance is placed, the greater will be the luminosity; and the more volatile the salt of the same metal, the more intense is the light produced.

Heated to the point of incandescence in any other manner, the vapours of these metals and their salts give out the same coloured light. Thus, if we burn gun-cotton, or gun-paper, steeped in solutions of these various salts, we get the characteristic colours. The well-known coloured fires owe their peculiar effects to the ignition of the vapour of some particular substance. Thus, in red fire we have strontium, in green fire we have barium salts present in the state of luminous vapour.

These facts have long been known and applied; but it was reserved for Bunsen and Kirchhoff to place these beautiful phenomena in their true position, to apply to them the modern methods of exact research; and thus to open out a new and rich field for most important investigations. This they accomplished in a most simple and beautiful manner, by examining these coloured flames, not by the naked eye, but by means of a prism or an apparatus for separating, decomposing, or splitting up the light produced by the incandescent vapour into its different constituent parts.

If we pass white sun-light through a prism, we get the well-known solar spectrum discovered by Newton. The red, or least refrangible rays appear at one end, and we pass through all gradations of colour—noticing on our way certain dark lines or spaces, showing the absence in solar light of some particular rays, lines with which we shall have much to do—until we arrive at the violet, or most refrangible end of the spectrum. If instead of using white sun-light, we pass the rays from the yellow soda flame through the prism, we get the soda spectrum; and we find that instead of a continuous spectrum, all we see is one bright yellow line, showing that every kind of light except that bright yellow ray, is absent in the soda flame; or that the soda flame gives out only one *kind* of light.

And as each metal, sodium, potassium, lithium, calcium, strontium, barium, &c., communicates a distinct tint to flame, so each gives a distinct and characteristic spectrum, consisting of certain bright coloured lines, or bands of light of the most peculiar form and tint.

The actual spectra of these metals can be beautifully seen in the simple apparatus designed by Bunsen and Kirchhoff.

In each spectrum of these metals, the form, number, position, colour, and tone of the bright lines remain perfectly constant and unvarying, so that from the presence or absence of one of these lines, we may with absolute certainty draw conclusions respecting the presence or absence of the particular metal, as we know of no two substances which produce the same bright lines. None of the bright lines produced by any one metal interfere in the least with those of any other, and in a mixture of all these metallic salts together, each ingredient can thus be easily detected.

As an example of the exactitude with which a very small quantity of a most complicated mixture can thus be analyzed, the speaker quoted Bunsen's words. "I took," says Bunsen, "a mixture of chloride of sodium, chloride of potassium, chloride of lithium,

chloride of calcium, chloride of strontium, chloride of barium, containing at most $\frac{1}{100,000}$ part of a grain of each substance. This mixture I put into the flame, and observed the result. First, the intense yellow sodium line appeared, on a background of a pale continuous spectrum; as this began to be less distinct, the pale potassium lines were seen, and then the red lithium line came out, whilst the barium lines appeared in all their vividness. The sodium, lithium, potassium, and barium salts were now almost all volatilized, and after a few moments the strontium and calcium lines came out, as from a dissolving view, gradually attaining their characteristic brightness and form."

We can thus detect the most minute traces of any one of these bodies, if mixed with the largest quantities of any other substance. The delicacy and accuracy of these reactions is without parallel, as is seen from the following statements:—

1. *Soda* $\frac{1}{3,000,000}$ part of a milligramme, or $\frac{1}{100,000,000}$ part of a grain of soda can be detected. Soda is always present in the air. All bodies exposed to air show the yellow sodium line. If a book be dusted near the flame the soda light can be seen.

2. *Lithia* $\frac{1}{100,000}$ part of a milligramme, or $\frac{1}{60,000,000}$ part of a grain of lithia can easily be detected. Lithium was only known to occur in four minerals. It is now found by spectrum analysis to be one of the most widely distributed elements. It exists in almost all rocks; it has been found in 3 cubic inches of sea, river, and Thames water; in the ashes of tobacco, and most plants; in milk, human blood, and muscular tissue.

3. *Strontia* $\frac{1}{100,000}$ of a milligramme, or $\frac{1}{1,000,000}$ parts of a grain of strontia can easily be detected.

4. *Lime* $\frac{1}{100,000}$ of a milligramme, or $\frac{1}{1,000,000}$ of a grain may be easily detected.

In examining the spectra of the alkalis obtained from certain mineral waters, Bunsen observed the occurrence of two bright blue lines which he had not seen before, when he examined alkalis from other sources. Hence he concluded that these bright lines must be produced by a new, hitherto undetected, alkaline metal. Subsequent search proved the validity of the supposition. The new metal was found and isolated. The analogy between this discovery and a celebrated one in another branch of physical science, will be at once understood. As Adams and Leverrier discovered Neptune, so Bunsen discovered "*Cæsium*" by the perturbations produced in the spectra of the other alkaline metals.

This is, however, not all. A few days ago the speaker received a letter from Bunsen, which contains the following most interesting information:—"The substance which I sent you as impure tartrate of Cæsium contains a *second* new alkaline metal. I am at present engaged in preparing its compounds. I hope soon to be able to give you more

detailed information concerning it. The spectrum of the new metal consists of two splendid red lines situated beyond the red line $K\alpha$ in the ultra red portion of the solar spectrum. Hence I propose to call the new metal '*Rubidium*.'

That this same method of investigation can be extended to all the metallic elements is more than probable, for Kirchhoff writes—"I have assured myself that even the metals of the rarest earths, as yttrium, erbium, and terbium, can be most quickly and certainly determined by help of the spectrum analytical method."

Experiments are being carried on with the view of making this mode of examination practically applicable to all metals.

To turn, now, to the second, and, if possible, to the more interesting part of the subject, namely, the conclusions drawn from these observations respecting the composition of the sun's atmosphere. The solar spectrum invariably contains a large number of dark lines, or spaces, or shadows. These have been called Fraunhofer's Lines, from the name of their discoverer. They show us that in the sun's light certain kind of rays are wanting; and as these lines are always present, exactly in the same position, we see that certain kinds of rays are always absent in solar light. There are many thousands of these lines in the whole length of the spectrum. Only a few have been, as yet, mapped and named.

What is the cause of these constant dark lines? And we must remember that it is in sunlight alone that these particular lines occur; in the light of the fixed stars, as well as in artificial lights, other lines are found. It is the discovery of this cause by Kirchhoff which gives the subject such peculiar interest, as it enables us to draw conclusions respecting the composition of the sun's atmosphere. The points of the case are put as concisely as possible under the following heads:—

1. The solar spectrum invariably contains certain fixed *dark* lines, called Fraunhofer's Lines.

2. The spectra produced by the luminous vapour of all metals contain certain fixed *bright* lines, invariable, and distinct for each metal.

3. All and each of the bright lines thus produced by certain metals—viz. sodium, potassium, magnesium, and iron—are found to coincide exactly with certain of the dark lines of the solar spectrum.

4. Hence there must be some connection between the bright lines of the metal, and the dark solar lines.

5. The connection is as follows:—Each of the dark fixed lines in the solar spectrum is caused by the presence in the sun's atmosphere of the luminous vapour of that metal which gives the coincident bright line.

By taking a special case we may more easily understand the matter. Let us examine the question why it is to be concluded that *Sodium* occurs in the sun's atmosphere? In the following sentences the reasoning on this subject is rendered clear:—

1. The light emitted by luminous sodium vapour is homogeneous. The sodium spectrum consists of one double bright yellow line.

2. This bright double sodium line is exactly coincident with Fraunhofer's dark double line D.

3. The spectrum of a Drummond's Light (like that of all incandescent solids) is continuous. It contains no dark lines or spaces.

4. If between the prism and the Drummond's Light a soda flame be placed, a dark double line identical with Fraunhofer's dark double line D is produced.

5. If instead of using Drummond's Light we pass sunlight through the soda flame, we see that the line D becomes much more distinct than when sunlight alone is employed.

6. The sodium flame has, therefore, the power of absorbing the same kind of rays as it emits. It is opaque for the yellow "D" rays.

7. Hence we conclude that luminous sodium vapour in the sun's atmosphere causes Fraunhofer's dark double line D. The light given off from the sun's solid body producing a continuous spectrum.

8. In a similar manner the presence in the solar atmosphere of potassium, iron, magnesium, nickel, and chromium has been proved.

Kirchhoff's own words may perhaps render this matter still more plain. "The sun," says Kirchhoff, "consists of a glowing gaseous atmosphere, surrounding a solid nucleus which possesses a still higher temperature. If we could see the spectrum of the solar atmosphere without that of the solid nucleus, we should notice in it the bright lines which are characteristic of the metals it contains. The more intense luminosity of the internal nucleus does not, however, permit the spectrum of the solar atmosphere to become apparent; it is *reversed* according to my newly discovered proposition; so that, instead of the *bright* lines which the luminous atmosphere by itself would have shown, *dark* ones appear. We do not see the spectrum of the solar atmosphere itself, but a negative image of it. This case, however, with an equal degree of certainty serves to detect the metals present in the sun's atmosphere. All that we require for this purpose is a very accurate knowledge of the solar spectrum, and of the spectra of the individual metals."

Kirchhoff is at present engaged in continuing these observations; and although only eighteen months have elapsed since the first discovery was made, he has already mapped more than seventy lines in the solar spectrum, between D and E, which are produced by iron. He has shown that the well-known group in the green, known as *b*, is caused by magnesium, whilst other coincident lines prove the presence of nickel, chromium, potassium, and sodium in the solar atmosphere.

The speaker regretted that he was unable to show even a drawing of these coincident lines, as no representation of them has yet been completed.

The lines produced by many metals possessing very distinctly marked spectra are seen to coincide with *none* of the dark solar lines;

and hence the conclusion is drawn, that these metals—for instance, silver, copper, zinc, aluminium, cobalt, lead, and antimony—do not occur at all, or at any rate occur only in very small quantities in the sun's atmosphere.

The speaker said that he should not soon forget the impression produced on his mind when visiting his friends in Heidelberg last autumn, by seeing the splendid spectacle of the coincidence of the bright lines of the iron spectrum with the dark solar lines. In the lower half of the field of the telescope were at least seventy brilliant iron lines of various colours, and of all degrees of intensity and of breadth; whilst in the upper half of the field, the solar spectrum, cut up, as it were, by hundreds of dark lines, exhibited its steady light. Situated *exactly* above each of the seventy bright iron lines was a dark solar line. These lines did not only coincide with a degree of sharpness and precision perfectly marvellous, but the intensity and breadth of each bright line was so accurately preserved in its dark representative, that the truth of the assertion that iron was contained in the sun, flashed upon the mind at once.

The speaker concluded by remarking that these researches are still in their earliest infancy; that the dawn of a new stellar and terrestrial chemistry has been announced, thus opening out for investigation a bright prospect of vast fields of unexplored truth.

[II. E. R.]

Friday, March 8, 1861.

The Rev. JOHN BARLOW, M.A. F.R.S. Vice-President,
in the Chair.

EDWARD FRANKLAND, F.R.S.

On some Phenomena attending Combustion in Rarefied Air.

THE investigation forming the subject of this discourse had its origin in some experiments which the speaker made upon the summit of Mont Blanc, in the autumn of 1859, for the purpose of ascertaining the effect of atmospheric pressure upon the amount of combustible matter consumed by a common candle. He found, as the average of five experiments, that a stearin candle diminished in weight 9·4 grammes, when burnt for an hour at Chamonix; whilst it consumed 9·2 grammes, when ignited for the same length of time on the summit of Mont Blanc. This close approximation in the quantity of combustible matter consumed under such widely different atmospheric pressures, goes far to prove that the rate of combustion is entirely independent of the density of the atmosphere. This result was subsequently confirmed by a repetition of the experiments in air, artificially rarefied, until it supported a column of only 9 inches of mercury.

In burning the candles upon the top of the mountain, it was noticed, in the subdued light of the tent in which the operation was performed, that their luminosity was much less than usual. The lower and blue portion of the flame, which, under ordinary circumstances, scarcely rises to within a quarter of an inch of the apex of the wick, now extended to the height of one-eighth of an inch above the cotton, thus greatly reducing the size of the luminous portion of the flame; and, on subsequently repeating the experiments in artificially rarefied atmospheres, and measuring the amount of light emitted in each case, it was found that as the rarefaction proceeded, the blue or non-luminous portion of the flame gradually extended upwards until it finally expelled, as it were, the yellow or luminous part even from the apex of the flame. During the progress of the

rarefaction, the flame became somewhat enlarged, assumed an ellipsoidal shape, and ultimately became almost globular, whilst a large external shell of bluish pink flame gradually came into view as the last portion of yellow light was disappearing from the apex of the flame, which had alone been previously visible. It is scarcely necessary to add, that during these changes in the flame, the light underwent a rapid diminution; the rate of its decrease, however, was subject to considerable irregularities from the heating of the apparatus surrounding the candle, and the consequent guttering and unequal combustion of the latter. For the accurate measurement of the diminution of light, therefore, recourse was had to coal gas, which, although also liable to certain disturbing influences, yet yielded results, during an extensive series of experiments, exhibiting sufficient uniformity to render them worthy of confidence.

By passing the gas through a "governor," uniformity of pressure in the delivery tubes could be secured; and by other appropriate arrangements a uniform amount of gas, viz. 0.65 cubic feet per hour, was made to burn in each experiment within the atmosphere of varying density. This experimental flame was placed at one extremity of a Bunsen's photometer; whilst, as a standard for comparison, a similar jet of gas, surrounded by a glass shade, and burning freely in the air with a uniform consumption, was fixed at the opposite end of the photometer. In the case of the experimental flame, the products of combustion were completely removed, and a steady supply of fresh air constantly supplied.

The following table contains a summary of the results of these determinations, the illuminating power given under each pressure being the average of twenty closely accordant observations. In each series the maximum illuminating effect—that is, the light given by the experimental flame when burning under the full atmospheric pressure—is taken at 100.

FIRST SERIES.

Pressure of Air in inches of Mercury.	Illuminating Power of Experimental Flame.	
	Observed.	Calculated.
29.9	100	100
24.9	75.0	74.5
19.9	52.9	49.0
14.6	20.2	22.0
9.6	5.4	3.5
6.6	.9	-18.8

SECOND SERIES.

Pressure of Air in inches of Mercury.	Illuminating Power of Experimental Flame.	
	Observed.	Calculated.
30·2	100	100
28·2	91·4	89·8
26·2	80·6	79·6
24·2	73·0	69·4
22·2	61·4	59·2
20·2	47·8	49·0
18·2	37·4	38·8
16·2	29·4	28·6
14·2	19·8	18·4
12·2	12·5	8·2
10·2	3·6	-2·0

An inspection of these results indicates that even the natural oscillations of atmospheric pressure cause a considerable variation in the amount of light emitted by gas flames. In order to determine these variations, the following special series of experiments was made, the pressures being very accurately ascertained by means of a water-gauge.

THIRD SERIES.

Pressure of Air in inches of Mercury.	Illuminating Power of Experimental Flame.	
	Observed.	Calculated.
30·2	100	100
29·2	95·0	94·9
28·2	89·7	89·8
27·2	84·4	84·7

It is thus evident that the combustion of an amount of gas which would give a light equal to 100 candles, when the barometer stands at 31 inches, would afford a light equal to only 84·4 candles if the barometer fell to 28 inches.

The results of these three series of observations taken together, show, that beginning at atmospheric pressure, and with 100 units of light, a decrease of almost exactly 5·1 units of light is the result of each diminution of mercurial pressure to the extent of one inch, until the barometer stands at 14 inches, below which the diminution of

light takes place in a less rapid ratio. One of the columns headed "Calculated," in the above tables, exhibits the illuminating power calculated from the constant just given, and it will be seen, that these calculated numbers nearly coincide in most cases with the observed amount of light.

In explaining the cause of the above phenomena, the speaker referred to the conditions upon which the light of ordinary flames depends. He showed, that in these flames there are two sources of light, viz. incandescent gaseous matter, and incandescent solid matter; but that, practically, 99 per cent. of the light of such flames owed its origin to the second of these sources. In gas, candle, and oil flames, the incandescent solid matter consisted of carbon in a minute state of division. The amount of light emitted by these flames depended, within certain limits, first, upon the quantity of solid particles of carbon existing, at any given moment, within the flame; and secondly, upon the temperature to which these carbon particles were heated. Now, the temperature of a flame might be affected by imperfect combustion in rarefied air; but it had been proved, by the analysis of the products, that combustion was equally complete in the above experiments under all pressures; in fact, it was found that complete combustion could be far more easily secured in rarefied air, than in air at the ordinary atmospheric pressure. Other experiments also showed, that the temperature of a flame was not materially affected by the pressure of the air in which it was burning; consequently, it was inferred that the diminution of luminosity in rarefied atmospheres, was not due either to imperfect combustion, or to reduction of temperature.

The diminution of light must therefore arise from the decrease of the amount of solid carbon separated within the flame; and this the speaker believed to be due to the admission of oxygen in larger quantities into the interior of the flame when the atmosphere was rarefied. It was shown by experiment, that the admission of a comparatively small amount of air, and consequently of oxygen, into the interior of a gas flame, immediately reduced the illuminating power of the latter to a very marked extent; the carbon particles, instead of being separated as such in the interior of the flame, being at once oxidized to carbonic oxide. This increased access of oxygen to the interior of a flame burning in rarefied air, was believed to be due to the greater mobility of the particles of expanded gases, which enabled the gases of the flame and the circumambient air to commingle more rapidly than at ordinary atmospheric pressure.

The cause of the less rapid decrease of the light of flames burning in atmospheres below 14 inches of mercurial pressure was due to the comparative prominence assumed by the light of the incandescent gaseous matters of the flame at such high stages of rarefaction; this gaseous illumination being affected by pressure to a much less extent than that afforded by incandescent carbon particles.

In his celebrated researches on flame, Davy had not overlooked the

diminution of light by decrease of pressure, but he had not determined the diminution quantitatively nor indicated its cause.

The speaker stated in conclusion, that he had only yet imperfectly extended his inquiry to pressures higher than that of the atmosphere ; but, so far as these experiments went, they appeared to indicate that the law which had been elicited for lower pressures, also held good for pressures above that of the atmosphere.

[E. F.]

Friday, March 15, 1861.

SIR HENRY HOLLAND, Bart. M.D. F.R.S. in the Chair.

LATIMER CLARK, Esq.

On Electrical Quantity and Intensity.

THE modifications of the strength of the electric current in dynamic electricity, and in the amount of charge in static electricity, are at present usually defined by the terms Quantity and Intensity. The speaker pointed out that the expression intensity, as ordinarily understood, really involved two perfectly distinct qualities, and dwelt on the advantage which would accrue to electrical science by the habitual separation of the complex idea of intensity into its two component parts, viz. that of *tension*, as propounded by Ohm in his celebrated mathematical investigation of the galvanic circuit; and that of *quantity*, as developed by Faraday in those valuable researches in which he established the definite quantitative character of electro-chemical decomposition and the action of electricity on the galvanometer. The term "tension," as here used, is intended to convey the same idea as the expression *electromotive force*, or as the term "electric potential," employed by Green and other mathematicians, and is entirely dissociated from the idea of quantity; both terms are equally applicable to electricity at rest or in motion.

The *quantity* of electricity, both in its static condition and in its motion through conductors, usually varies directly as the tension, and hence their joint effects have been ordinarily confounded together and attributed to one cause under the name of intensity; but since the tension and quantity do not, under all circumstances, vary in the same ratio, there exists an absolute necessity for their clear separation before any numerical reasoning can be founded on them. Cases of the independent variation of tension and quantity were shown, and it was pointed out that all the most striking properties of electricity, such as the decomposition of water and salts, the combustion of metals, the deflection of the galvanometer, the attraction of the electro-magnet, and the physiological effects of the current were really dependent, as regards their magnitude and energy, solely on the quantity of elec-

tricity passing. Their greater energy when the tension was increased, was an indirect effect, due not to that tension, but to the increased quantity which passed in a given time by reason of the increased tension. A galvanometer wound with a few turns of thick wire was shown to be deflected as powerfully by one cell as by six, or even by 600 cells of the same size, because by reason of its shortness the wire conveyed freely the whole quantity which one cell could produce, which was the same as that produced by the whole 600; but any alteration in the size of the cell produced a consequent change in the quantity and in the deflection of the galvanometer. On the other hand, a galvanometer with many thousand turns of fine wire gave the same deflection with a battery formed of a small gun-cap, as with one of twenty square feet of surface, because the quantity in this case was regulated and limited not by the size of the plates, but by the power of conduction of the wire; the quantity being therefore the same in both cases. In every case the deflection was dependent solely on the quantity of electricity actually passing through the instrument without reference to its tension.

The combustion of metals was shown to be a phenomenon dependent on quantity, and not on tension; one cell of Grove's battery ignited a certain length of platina wire; and whatever its size it would ignite no greater length; but two, three, or more cells were shown to ignite two and three times the original length, the quantity passing in the greater length being under the higher tension, precisely the same as in the original length. This explained Faraday's oft misunderstood remark, that the same quantity of electricity which would ignite an inch of wire, would ignite a foot or a mile.

The pain and shock experienced on touching a powerful battery, or shocking coil, or Leyden jar, were proportionate to the quantity of electricity passing through the system, and not to the tension. A carrier ball or minute Leyden jar charged to the highest tension, would produce no sensation if the quantity were absent, and the same was the case with a Zamboni's pile. Sparks nearly eighteen inches long were received from an electrical machine; but although of this high tension, they produced no violent physiological effects, owing to their deficiency in quantity. Long sparks, nevertheless, produced greater effects than short ones, because at double the striking distance the tension is doubled, and the quantity is therefore twice as great. The quantity contained in a Leyden jar or battery is comparatively great, and the effect on the system proportionately violent. Two conditions were necessary for these effects: first, that the quantity present should be considerable; and secondly, that the tension should be sufficiently great to make it pass through the system. A battery of two or three cells, which could readily fuse platina wire, was shown to produce no painful sensation on the tongue, because, although the quantity was abundant, the tension was low; while another battery of 600 cells, which produced the most intolerable shock to the system, had, from its deficiency in quantity, scarcely any power to fuse wire. The

Ruhmkorff coil combined very high tension with considerable quantity, and its physiological effects were therefore very violent.

A frictional machine was exhibited by Mr. Varley, constructed on a plan of Dr. Winter's; the plate was of vulcanite, or vulcanized India rubber, about three feet in diameter, excited by amalgam in the usual way; its peculiarity was a large and lofty wooden ring, with a metallic rod in its interior, which, by its overshadowing inductive influence, increased the length of the sparks from six or seven inches to nearly eighteen.

The forces of electrical attraction and repulsion are sometimes stated to vary as the square of the intensity, sometimes as the square of the quantity, and sometimes as the square of the distance; but it was contended, that these effects were due to the circumstance that the quantity usually varies in the same ratio as the tension, and as the distance; and that all the phenomena were more rationally explained by the assumption that electrical attraction and repulsion vary in the simple ratio of the quantity and of the tension, and of the distance inversely.

The instances in which the quantity present is not simply dependent on the tension, are those in which other electrified bodies are present, which, by their inductive influence, affect the quantity present in all bodies in their vicinity without necessarily affecting their tension. An insulated cylinder was connected with the positive pole of a Daniell's battery of 600 cells, its negative pole being connected with the earth; so that the cylinder was in a condition to give off a powerful and visible current to another wire connected with the ground; in this condition a positively electrified disc was approached to it, and by its inductive influence was shown to render one end of the cylinder electrically negative, so that a carrier ball applied to that end showed it to have a negative charge, thus presenting the apparent paradox of a negative electrified body giving off a positive current to the earth, or *vice versâ*. One end was negatively electrified, and the other end positively, but the tension was the same everywhere.

According to the ordinary way of regarding this class of phenomena, it was usual to state that the ends of the cylinder acquired a state of positive or negative *intensity*, or that they had their intensity changed: it was contended that this gave an inaccurate idea of the real nature of the change, and that the approach of an electrified body, however near or however violently it might be excited, could not in the slightest degree affect the tension of a conducting body, which was in connection with the earth: the only influence it could have would be to alter the *quantity* in the second body, by driving a portion of its electricity downwards to the earth. It might be assumed as a law that *the tension of the electricity in every part of a conducting body of moderate dimensions was the same*, notwithstanding the vicinity of other electrified bodies. If a positively electrified body were brought near an insulated conductor, the distribution of the electricity in the second body was changed, and its whole tension was raised, but the tension

remained everywhere uniform, and was as high at the negative as at the positive end.

The fall of tension in electricity was always accompanied by its conversion into heat; the ignition of wire by the voltaic current, the intense heat of the voltaic arc, and the heat and light of the electric discharge and of the spark, were all cases of the evolution of heat consequent on the fall of tension, and the quantity of heat evolved was apparently *directly proportionate to the fall of tension within a given space and to the quantity of electricity passing.*

In the case of electric telegraph conductors and submarine cables, it was shown, from a carefully conducted and extensive series of experiments, that the tension falls with the most perfect regularity from the positive pole of the battery to the end in connection with the earth, in accordance with the law of Ohm; and since the quantity of electricity held under induction varies in the same ratio as the tension, the distribution of the charge in a cable follows precisely the same law. It results from this, that if a cable with a current flowing through it be divided into any number of equal sections, and the quantity in the section connected with the earth be taken as unity, the quantities in all the other parts, whatever their number, will be in the ratio 1, 3, 5, 7, 9, 11, &c. So that if a cable be divided into two halves, the quantities will be in the ratio of one to three.

The speaker stated that he had ascertained that in the voltaic battery, the presence of two metals was not an essential condition—the negative metal was not necessary for the formation of the electric current, but only for its after detection and exhibition. A simple mass of copper, iron, zinc, or any oxidisable metal, when laid on the moist earth, formed a complete battery in itself, giving positive electricity to the earth, and quickly assuming a negative tension, which it would communicate to any other body resting on it or in contact with it, as, for instance, a length of submarine cable. If a cable thus charged were afterwards removed and applied to any more electro-negative metal, such as platinum, or to carbon, the charge would return to the earth; and it was found by measurement that the charge thus acquired by a cable was exactly the same as if the two metals had been employed simultaneously in the ordinary form of a galvanic couple. If the mass of zinc were permanently connected with the non-oxidisable or less oxidisable metal, and thus with the earth, the tension, being constantly destroyed and as constantly renewed, would form a constant current, becoming in fact a voltaic couple. If the connection with the earth, instead of being made through another metal, were made through any inert conducting substance or liquid, the same constant current would be produced, thus forming the well-known case of a voltaic battery with one metal and two liquids.

After the conclusion of the discourse, Mr. Clark stated, with reference to Faraday's discovery of specific inductive capacity, that in the course of some investigations conducted in conjunction with Professor Hughes, they had observed that every different dielectric possessed

its own specific law of variation of inductive capacity with respect to distance. With air it varied as the distance, inversely; but with gutta-percha it was more nearly as the square root; with India-rubber and white wax it was intermediate between the two ratios, and the law of variation was different with every substance tried. From which it would result that observers who deduced the specific inductive capacity of any material from experiments on half-inch plates, would arrive at very different results from others who operated on inch plates.

[L. C.]

Friday, April 12, 1861.

WILLIAM ROBERT GROVE, Esq. M.A. Q.C. F.R.S. Vice-President,
in the Chair.

PROFESSOR HELMHOLTZ, F.R.S.

*On the Application of the Law of the Conservation of Force to
Organic Nature.*

THE most important progress in natural philosophy by which the present century is distinguished, has been the discovery of a general law which embraces and rules all the various branches of physics and chemistry. This law is of as much importance for the highest speculations on the nature of forces, as for immediate and practical questions in the construction of machines. This law at present is commonly known by the name of "the principle of conservation of force." It might be better perhaps to call it, with Mr. Rankine, "the conservation of energy," because it does not relate to that which we call commonly *intensity* of force; it does not mean that the intensity of the natural forces is constant: but it relates more to the whole amount of power which can be gained by any natural process, and by which a certain amount of work can be done. For example: if we apply this law to gravity, it does not mean, what is strictly and undoubtedly true, that the intensity of the gravity of any given body is the same as often as the body is brought back to the same distance from the centre of the earth. Or with regard to the other elementary forces of nature—for example, chemical force: when two chemical elements come together, so that they influence each other, either from a distance or by immediate contact, they will always exert the same force upon each other—the same force both in intensity and in its direction and in its quantity. This other law indeed is true; but it is not the same as the principle of conservation of force. We may express the meaning of the law of conservation of force by saying, that every force of nature when it effects any alteration, loses and exhausts its faculty to effect the same alteration a second time. But while, by every alteration in nature, that force which has been the cause of this alteration is exhausted, there is always another force which gains as much power of producing new alterations in nature as the first has lost. Although, therefore, it is the nature of all inorganic forces to become exhausted by their own working, the power of the whole system in which these alterations take place is neither exhausted nor increased in quantity, but only

changed in form. Some special examples will enable you better to understand this law than any general theories. We will begin with gravity; that most general force, which not only exerts its influence over the whole universe, but which at the same time gives the means of moving to a great number of our machines. Clocks and smaller machines, you know, are often set in motion by a weight. The same is really the case with water-mills. Water-mills are driven by falling water; and it is the gravity, the weight of the falling water, which moves the mill. Now you know that by water-mills, or by a falling weight, every machine can be put in motion; and that by these motive powers every sort of work can be done which can be done at all by any machine. You see, therefore, that the weight of a heavy body, either solid or fluid, which descends from a higher place to a lower place is a motive power, and can do every sort of mechanical work. Now if the weight has fallen down to the earth, then it has the same amount of gravity, the same intensity of gravity; but its power to move, its power to work, is exhausted; it must become again raised before it can work anew. In this sense, therefore, I say that the faculty of producing new work is exhausted—is lost; and this is true of every power of nature when this power has produced alteration. Hence, therefore, the faculty of producing work, of doing work, does not depend upon the intensity of gravity. The intensity of gravity may be the same, the weight may be in a higher position or in a lower position, but the power to work may be quite different. The power of a weight to work, or the amount of work which can be produced by a weight, is measured by the product of the height to which it is raised and the weight itself. Therefore our common measure is foot-pound; that is, the product of the number of feet and the number of pounds. Now we can by the force of a falling weight raise another weight; as, for example, the falling water in a water-mill may raise the weight of a hammer. Therefore it can be shown that the work of the raised hammer, expressed in foot-pounds, that is, the weight of the hammer multiplied by the height expressed in feet to which it is raised, that this amount of work cannot be greater than the product of the weight of water which is falling down, and the height from which it fell down. Now we have another form of motive power, of mechanical motive power; that is, velocity. The velocity of any body in this sense, if it is producing work, is called *vis viva*, or living force, of that body. You will find many examples of it. Take the ball of a gun. If it is shot off, and has a great velocity, it has an immense power of destroying; and if it has lost its velocity, it is quite a harmless thing. The great power it has depends only on its velocity. In the same sense, the velocity of the air, the velocity of the wind, is motive power; for it can drive windmills, and by the machinery of the windmills it can do every kind of mechanical work. Therefore you see that also velocity in itself is a motive force.

Take a pendulum which swings to and fro. If the pendulum is raised to the side, the weight is raised up; it is a little higher than

when it hangs straightly down, perpendicular. Now if you let it fall, and it comes to its position of equilibrium, it has gained a certain velocity. Therefore, at first, you had motive power in the form of a raised weight. If the pendulum comes again to the position of equilibrium, you have motive power in the form of *vis viva*, in the form of velocity, and then the pendulum goes again to the other side, and it ascends again till it loses its velocity; then again, *vis viva* or velocity is changed into elevation of the weight: so you see in every pendulum that the power of a raised weight can be changed into velocity, and the velocity into the power of a raised weight. These two are equivalent.

Then take the elasticity of a bent spring. It can do work, it can move machines or watches. The cross-bow contains such springs. These springs of the watch and cross-bow are bent by the force of the human arm, and they become in that way reservoirs of mechanical power. The mechanical power which is communicated to them by the force of the human arm, afterwards is given out by a watch during the next day. It is spent by degrees to overpower the friction of the wheels. By the cross-bow, the power is spent suddenly. If the instrument is shot off, the whole amount of force which is communicated to the spring is then again communicated to the shaft, and gives it a great *vis viva*.

Now the elasticity of air can be a motive power in the same way as the elasticity of solid bodies; if air is compressed, it can move other bodies; let us take the air-gun; there the case is quite the same as with the cross-bow. The air is compressed by the force of the human arm; it becomes a reservoir of mechanical power; and if it is shot off, the power is communicated to the ball in the form of *vis viva*, and the ball has afterwards the same mechanical power as is communicated to the ball of a gun loaded with powder.

The elasticity of compressed gases is also the motive power of the mightiest of our engines, the steam-engine; but there the case is different. The machinery is moved by the force of the compressed vapours, but the vapours are not compressed by the force of the human arm, as in the case of the compressed air-gun. The compressed vapours are produced immediately in the interior of the boiler by the heat which is communicated to the boiler from the fuel.

You see, therefore, that in this case the heat comes in the place of the force of the human arm, so that we learn by this example, that heat is also a motive power. This part of the subject, the equivalence of heat as a motive power, with mechanical power, has been that branch of this subject which has excited the greatest interest, and has been the subject of deep research.

It may be considered as proved at present, that if heat produces mechanical power, that is, mechanical work, a certain amount of heat is always lost. On the other hand, heat can be also produced by mechanical power, namely, by friction and the concussion of unelastic bodies. You can bring a piece of iron into a high temperature, so

that it becomes glowing and luminous, by only beating it continuously with a hammer. Now, if mechanical power is produced by heat, we always find that a certain amount of heat is lost; and this is proportional to the quantity of mechanical work produced by that heat. We measure mechanical work by foot-pounds, and the amount of heat we measure by the quantity of heat which is necessary to raise the temperature of one pound of water by one degree, taking the centigrade scale. The equivalent of heat has been determined by Mr. Joule, of Manchester. He found that one unit of heat, or that quantity of heat which is necessary for raising the temperature of a pound of water one degree centigrade, is equivalent to the mechanical work by which the same mass of water is raised to $423\frac{1}{2}$ metres, or 1389 English feet. This is the mechanical equivalent of heat.

Hence, if we produce so much heat as is necessary for raising the temperature of one pound of water by one degree, then we must apply an amount of mechanical work equal to raising one pound of water 1389 English feet, and lose it for gaining again that heat.

By these considerations, it is proved, that heat cannot be a ponderable matter, but that it must be a motive power, because it is converted into motion or into mechanical power, and can be either produced by motion or mechanical power. Now, in the steam-engine we find that heat is the origin of the motive power, but the heat is produced by burning fuel, and therefore the origin of the motive power is to be found in the fuel, that is, in the chemical forces of the fuel, and in the oxygen with which the fuel combines.

You see from this, that the chemical forces can produce mechanical work, and can be measured by the same units and by the same measures as any other mechanical force. We may consider the chemical forces as attractions, in this instance, as attraction of the carbon of the fuel for the oxygen of the air; and if this attraction unite the two bodies, it produces mechanical work just in the same way as the earth produces work, if it attract a heavy body. Now the conservation of force, of chemical force, is of great importance for our subject to-day, and it may be expressed in this way. If you have any quantity of chemical materials, and if you cause them to pass from one state into a second state, in any way, so that the amount of the materials at the beginning, and the amount of the materials at the end of this process be the same, then you will have always the same amount of work, of mechanical work or its equivalent, done during this process. Neither more nor less work can be done by the process. Commonly, no mechanical work in the common sense is done by chemical force, but usually it produces only heat; hence the amount of heat produced by any chemical process must be independent of the way in which that chemical process goes on. The way may be determined by the will of the experimenter as he likes.

We see, therefore, that the energy of every force in nature can be measured by the same measure, by foot-pounds, and that the energy of the whole system of bodies which are not under the influence of

any exterior body must be constant; that it cannot be lessened or increased by any change. Now the whole universe represents such a system of bodies endowed with different sorts of forces and of energy, and therefore we conclude from the facts I have brought before you, that the amount of working power, or the amount of energy in the whole system of the universe must remain the same, quite steady and unalterable, whatever changes may go on in the universe. If we accept the hypothesis of Laplace, that in the first state the universe was formed by a chaos of nebulous matter, spread out through infinite space, then we must conclude, that at this time the only form of energy existing in this system was the attraction of gravitation, and it was therefore the same sort of energy as is possessed by a raised weight. Afterwards, astronomers suppose, this nebulous matter was conglomerated and aggregated to solid masses. Great quantities of this nebulous matter, possibly from a great distance, fell together, and thus their attraction, or the energy of their attraction was destroyed, and hence heat must have been produced; and the facts we know at present are sufficient to enable us to calculate the amount of this heat, that is, of the whole heat which must have been produced during the whole process of conglomeration. This amount of heat is immensely great, so that it surpasses all our ideas and all the limits of our imagination. If we calculate this quantity of heat, and suppose that the sun contained at the same time the whole heat, and that the sun had the same specific heat as water, the sun would be heated to twenty-eight millions of degrees, that is, to a temperature surpassing all temperatures we know on earth; however, this temperature could not exist at any time in the sun, because the heat which was produced by the aggregation of the masses, must also be spent partially by radiation into space. I give only the result of these calculations, in order that you may see from it what a great amount of heat could be produced in this way. The same process goes on also at present in the falling stars and meteors which come down to the earth from planetary spaces. Their velocity is destroyed by the friction of the air and by the concussion with the surface of the earth, and we see how they become luminous, and if they are found on the earth, we find them hot.

The sun also at present is hotter than any heated body here on the earth. That is shown by the latest experiments made by Professors Kirchhoff and Bunsen, of Heidelberg, on the spectrum of the sun, by which it is proved, that in the atmosphere of the sun, iron and other metals are contained as vapours which cannot be changed into vapours by any amount of heat on the earth.

Our earth contains a great amount of energy in the form of its interior heat. This part of its energy produces the volcanic phenomena; but it is without great influence upon the phenomena of the surface, because only a very small amount of this heat comes through. It can be calculated that the amount of heat which goes from the interior to the surface cannot raise the temperature of the surface any higher than the thirteenth part of a degree.

We have another power which produces motion on the surface of the earth. I mean the attraction of the sun and of the moon producing the tides.

All the other phenomena on the surface of the earth are produced by the radiation of the sun, by the sunbeams ; and the greater part of those changes which occur on the surface of our earth, are caused by the heat of the sun. As the heat of the sun is distributed unequally over the surface, some parts of the atmosphere become heated more than other parts ; the heated parts of the atmosphere rise up, and so winds and vapours are produced. They come down at first as clouds in the higher parts of the atmosphere, and then as rain upon the surface of the earth ; they are collected as rivers, and go again down into the sea. So you see that all the meteorological phenomena of our earth are produced by the effect of the solar beams by the heat of the sun.

The light of the sun is the cause of another series of phenomena, and the principal products of the light of the sun are plants, because plants can only grow with the help of the sun-light. It is only by the help of the sun-light, that they can produce the inflammable matter which is deposited in the bodies of plants, and which is extracted from the carbonic acid and the water contained in the atmosphere, and in the earth itself.

This may give you an idea of the sense and bearing of the general principle on which I purpose to speak. As many English philosophers have been occupied with working out the consequences of this most general and important principle for the theory of heat, for the energy of the solar system, for the construction of machines, you will hear these results better explained by your own countrymen ; I shall abstain from entering farther into this part of the subject. At the same time that Mr. Grove showed that every force of nature is capable of bringing into action every other force of nature, Mr. Joule, of Manchester, began to search for the value of the mechanical equivalent of heat, and to prove its constancy, principally guided by the more practical interests of engineering. The first exposition of the general principle was published in Germany by Mr. Mayer, of Heilbron, in the year 1842. Mr. Mayer was a medical man, and much interested in the solution of physiological questions, and he found out the principle of the conservation of force guided by these physiological questions. At the same time also, I myself began to work on this subject. I published my researches a little later than Mr. Mayer, in 1845. Now, at first sight, it seems very remarkable and curious, that even physiologists should come to such a law. It appears more natural, that it should be detected by natural philosophers or engineers, as it was in England ; but there is, indeed, a close connection between both the fundamental questions of engineering and the fundamental questions of physiology with the conservation of force. For getting machines into motion, it is always necessary to have motive-power, either in water, fuel, or living animal matter. The constructors of machines, instruments,

watches, within the last century, who did not know the conservation of force, were induced to try if they could not keep a machine in motion without any expenditure for getting the motive power. Many of them worked for a long time very industriously to find out such a machine which would give perpetual motion, and produce any mechanical work which they liked. They called such a machine a perpetual mover. They thought they had an example of such a machine in the body of every animal. There, indeed, motive-power seemed to be produced every day without the help of any external mechanical force. They were not aware that eating could be connected with the production of mechanical power. Food they believed was wanted only to restore the little damages in the machine, or to keep off friction, like the fat which made the axles of wheels to run smoothly. Now at first by the mathematicians of the last century, the so-called principle of the conservation of *vis viva* was detected, and it was shown that by the action of the purely mechanical powers, it was not possible to construct a perpetual mover; but it remained still doubtful if it would not be possible to do so by the interposition of heat, or electricity, or chemical force. At last, the general law of conservation of force was discovered, and stated, and established; and this law shows that also by the connection of mechanical powers with heat, with electricity, or with chemical force, no such machine can be constructed to give a perpetual motion, and to produce work from nothing.

We must consider the living bodies under the same point of view, and see how it stands with them. Now if you compare the living body with a steam-engine, then you have the completest analogy. The living animals take in food that consists of inflammable substances, fat and the so-called hydrocarbons, as starch and sugar, and nitrogenous substances, as albumen, flesh, cheese, and so on. Living animals take in these inflammable substances and oxygen; the oxygen of the air, by respiration. Therefore, if you take, in the place of fat, starch, and sugar, coals or wood, and the oxygen of the air, you have the substances in the steam-engine. The living bodies give out carbonic acid and water; and then if we neglect very small quantities of more complicated matters which are too small to be reckoned here, they give up their nitrogen in the form of urea. Now let us suppose that we take an animal on one day, and on any day afterwards; and let us suppose that this animal is of the same weight the first day and the second day, and that its body is composed quite in the same way on both days. During the time—the interval of time—between these two days the animal has taken in food and oxygen, and has given out carbonic acid, water, and urea. Therefore, a certain quantity of inflammable substance, of nutriment, has combined with oxygen, and has produced nearly the same substances, the same combinations, which would be produced by burning the food in an open fire, at least, fat, sugar, starch, and so on; and those substances which contained no nitrogen would give us quite in the same way carbonic acid and water, if they are burnt in the open fire, as if they are burnt in the

living body ; only the oxidation in the living body goes on more slowly. The albuminous substances would give us the same substances, and also nitrogen, as if they were burnt in the fire. You may suppose, for making both cases equal, that the amount of urea which is produced in the body of the animal, may be changed without any very great development of heat, into carbonate of ammonia, and carbonate of ammonia may be burnt, and gives nitrogen, water, and carbonic acid. The amount of heat which would be produced by burning urea into carbonic acid and nitrogen, would be of no great value when compared with the great quantity of heat which is produced by burning the fat, the sugar, and the starch. Therefore we can change a certain amount of food into carbonic acid, water, and nitrogen, either by burning the whole in the open fire, or by giving it to living animals as food, and burning afterwards only the urea. In both cases we come to the same result.

Now I have said that the conservation of force for chemical processes requires a fixed amount of mechanical work, or its equivalent, to be given out during this process ; and the amount is exactly the same in whatever way the process may go on. And therefore we must conclude that by the animal as much work must be done, must be given out—the same equivalent of mechanical work—as by the chemical process of burning. Now let us remark that the mechanical work which is spent by an animal, and which is given to the external world, consists, firstly, in heat ; and secondly, in real mechanical work. We have no other forms of work, or of equivalent of work, given out by living animals. If the animal is reposing, then the whole work must be given out in the form of heat ; and therefore we must conclude that a reposing animal must produce as much heat as would be produced by burning its food. A small difference would remain for the urea ; we must suppose that the urea produced by the animal is also burnt, and taken together with the heat immediately produced by the animal itself. Now we have experiments made upon this subject by the French philosophers Dulong and Desprez. They found that these two quantities of heat—the one emitted by burning, the other by the living animal—are nearly identical ; at least, so far as could be established at that time, and with those previous researches which existed at that time. The heat which is produced by burning the materials of the food is not quite known even now. We want to have researches on the heat produced by the more complicated combinations which are used as food. Dulong and Desprez have calculated the heat according to the theoretical supposition of Lavoisier—which supposition is nearly right, but not quite right—therefore there is a little doubt as to the amount of the heat, but experiments show that at least to the tenth part of that heat the quantities are really equal ; and we may hope if we have better researches on the heat produced by burning the food, that these quantities will also be more equal than they were found to be by Dulong and Desprez.

Now if the body be not reposing, but if muscular exertion take

place, then also mechanical work is done. The mechanical work is very different according to the different kinds of muscular exertion. If we walk only on a plane surface, we must overpower the resistance of friction and the resistance of the air; but these resistances are not so great that the work which we do by walking on a plane is of great amount. Our muscles can do work in very different ways. By the researches of Mr. Redtenbacher, the director of the Polytechnic School of Carlsruhe, it is proved that the best method of getting the greatest amount of work from a human body is by the treadmill, that is, by going up a declivity. If we go up the declivity of a hill we raise the weight of our own body. In the treadmill the same work is done, only the mill goes always down, and the man on the mill remains in his place.

Now we have researches on the amount of air which is taken in and of carbonic acid given out during such work in the treadmill, made by Dr. Edward Smith. He found that a most astonishing increase of respiration takes place during such work. Now you all know that if you go up a hill you are hindered in going too fast by the great frequency and the great difficulty of respiration. This, then, becomes far greater than by the greatest exertion of walking on a plain, and really the difficulty is produced by the great mechanical work which is done in the same time. Now, partly from the experiments of Dulong and Desprez, and partly from the experiments of Dr. Edward Smith, we can calculate that the human body, if it be in a reposing state, but not sleeping, consumes so much oxygen, and burns so much carbon and hydrogen, that during one hour so much heat is produced that the whole body, or a weight of water equal to the weight of the body, would be raised in temperature one degree and two-tenths centigrade (two degrees and two-tenths Fahrenheit). Now Dr. Edward Smith found that by going in the treadmill at such a rate that if he went up a hill at the same rate, he would have risen during one hour 1712 feet, that during such a motion he exhaled five times as much carbonic acid as in the quiet state, and ten times as much as in sleeping. Therefore the amount of respiration was increased in a most remarkable way. If we now calculate these numbers we find that the quantity of heat which is produced during one hour of repose is one degree and two-tenths centigrade, and that these are nearly equivalent to rising 1712 feet, so that therefore the amount of mechanical work done in a treadmill, or done in ascending a hill at a good rate, is equivalent to the whole amount of heat which is produced in a quiescent state. The whole amount of the decomposition in the living body is five times as great as in a reposing and wakeful state. Of these five quantities, one quantity is spent for mechanical work, and four-fifths remain in the form of heat. Always in ascending a hill, or in doing great mechanical work, you become hot, and the production of heat is extremely great, as you well know, without making particular experiments. Hence you see how much the decomposition in the body is increased by doing really mechanical work.

Now these measurements give us another analogy. We see that in ascending a mountain we produce heat and mechanical work, and that the fifth part of the equivalent of the work which is produced by the chemical process is really gained as mechanical work. Now if we take our steam-engine, or a hot-air engine, or any other engine which is driven by heat in such a way that one body is heated and expands, and by the expansion other bodies are moved,—I say, if we take any thermo-dynamic engine, we find that the greatest amount of mechanical work which can be gained by chemical decomposition or chemical combination is only an eighth part of the equivalent of the chemical force, and seven-eighths of the whole are lost in the form of heat; and this amount of mechanical work can only be gained if we have the greatest difference of temperature which can be produced in such a machine. In the living body we have no great difference of temperature; and in the living body the amount of mechanical work which could be gained if the living body were a thermo-dynamic engine, like the steam-engine or the hot-air engine, would be much smaller than one-eighth. Really, we find from the great amount of work done, that the human body is in this way a better machine than the steam-engine, only its fuel is more expensive than the fuel of steam-engines.

There is another machine which changes chemical force into mechanical power; that is, the magneto-electric machine. By these magneto-electric machines a greater amount of electrical power can be changed into mechanical work than in our artificial thermo-dynamic machines. We produce an electric current by dissolving zinc in sulphuric acid, and liberating another oxidizable matter. Generally it is only the difference of the attraction of zinc for oxygen compared with the attraction of copper or nitrous acid for oxygen. In the human body we burn substances which contain carbon and hydrogen, and therefore the whole amount of attraction of carbon and hydrogen for oxygen is put into action to move the machine; and in this way the power of the living body is greater and more advantageous than the power of the magneto-electric machine.

Let us now consider what consequences must be drawn when we find that the laws of animal life agree with the law of the conservation of force, at least as far as we can judge at present regarding this subject. As yet we cannot prove that the work produced by living bodies is an exact equivalent of the chemical forces which have been set into action. It is not yet possible to determine the exact value of either of these quantities so accurately as will be done ultimately; but we may hope that at no distant time it may be possible to determine this with greater accuracy. There is no difficulty opposed to this task. Even at present I think we may consider it as extremely probable that the law of the conservation of force holds good for living bodies.

Now we may ask, what follows from this fact as regards the nature of the forces which act in the living body?

The majority of the physiologists in the last century, and in the beginning of this century, were of opinion that the processes in living

bodies were determined by one principal agent which they chose to call the "vital principle." The physical forces in the living body they supposed could be suspended or again set free at any moment, by the influence of the vital principle; and that by this means this agent could produce changes in the interior of the body, so that the health of the body would be thereby preserved or restored.

Now the conservation of force can exist only in those systems in which the forces in action (like all forces of inorganic nature) have always the same intensity and direction if the circumstances under which they act are the same. If it were possible to deprive any body of its gravity, and afterwards to restore its gravity, then indeed we should have the perpetual motion. Let the weight come down as long as it is heavy; let it rise if its gravity is lost; then you have produced mechanical work from nothing. Therefore this opinion that the chemical or mechanical power of the elements can be suspended, or changed, or removed in the interior of the living body, must be given up if there is complete conservation of force.

There may be other agents acting in the living body, than those agents which act in the inorganic world; but those forces, as far as they cause chemical and mechanical influences in the body, must be quite of the same character as inorganic forces, in this at least, that their effects must be ruled by necessity, and must be always the same, when acting in the same conditions, and that there cannot exist any arbitrary choice in the direction of their actions.

This is that fundamental principle of physiology which I mentioned in the beginning of this discourse.

Still at the beginning of this century physiologists believed that it was the vital principle which caused the processes of life, and that it detracted from the dignity and nature of life, if anybody expressed his belief that the blood was driven through the vessels by the mechanical action of the heart, or that respiration took place according to the common laws of the diffusion of gases.

The present generation, on the contrary, is hard at work to find out the real causes of the processes which go on in the living body. They do not suppose that there is any other difference between the chemical and the mechanical actions in the living body, and out of it, than can be explained by the more complicated circumstances and conditions under which these actions take place; and we have seen that the law of the conservation of force legitimizes this supposition. This law, moreover, shows the way in which this fundamental question, which has excited so many theoretical speculations, can be really and completely solved by experiment.

[H. H.]

Friday, June 7, 1861.

THE DUKE OF NORTHUMBERLAND, K.G. F.R.S. President,
in the Chair.

JOHN TYNDALL, Esq. F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION.

On the Physical Basis of Solar Chemistry.

OMITTING all preface, the speaker drew attention to an experimental arrangement intended to prove that gaseous bodies radiate heat in different degrees. Behind a double screen of polished tin was placed an ordinary ring gas-burner; on this was placed a hot copper ball, from which a column of heated air ascended: behind the screen, but so placed that no ray from the ball could reach the instrument, was an excellent thermo-electric pile, connected by wires with a very delicate galvanometer. The thermo-electric pile was known to be an instrument whereby heat was applied to the generation of electric currents; the strength of the current being an accurate measure of the quantity of the heat. As long as both faces of the pile were at the same temperature, no current was produced; but the slightest difference in the temperature of the two faces at once declared itself by the production of a current, which, when carried through the galvanometer, indicated by the deflection of the needle both its strength and its direction.

The two faces of the pile were in the first instance brought to the same temperature; the equilibrium being shown by the needle of the galvanometer standing at zero. The rays emitted by the current of hot air already referred to were permitted to fall upon one of the faces of the pile; and an extremely slight movement of the needle showed that the radiation from the hot air, though sensible, was extremely feeble. Connected with the ring-burner was a holder containing oxygen gas; and by turning a cock, a stream of this gas was permitted to issue from the burner, strike the copper ball, and ascend in a heated column in front of the pile. The result was, that oxygen showed itself, as a radiator of heat, to be quite as feeble as atmospheric air.

A second holder containing olefiant gas was also connected by its own system of tubes with the ring-burner. Oxygen had already flowed over the ball and cooled it in some degree. Hence, as a radiator in comparison with oxygen, the olefiant gas laboured under a disadvantage. It was purposely arranged that this should be the case; so that if, notwithstanding its being less hot, the olefiant gas showed itself a better radiator, its claim to superiority in this respect would be decisively proved. On permitting the gas to issue upwards,

it cast an amount of heat against the adjacent face of the pile sufficient to impel the needle of the galvanometer almost to its stops at 90° . This experiment proved the vast difference between two equally transparent gases with regard to their power of emitting radiant heat.

The converse experiment was now performed. The thermo-electric pile was removed and placed between two cubes filled with water kept in a state of constant ebullition; and it was so arranged that the quantities of heat falling from the cubes on the opposite faces of the pile were exactly equal, thus neutralizing each other. The needle of the galvanometer being at zero, a sheet of oxygen gas was caused to issue from a slit between one of the cubes and the adjacent face of the pile. If this sheet of gas possessed any sensible power of intercepting the thermal rays from the cube, one face of the pile being deprived of the heat thus intercepted, a difference of temperature between its two faces would instantly set in, and the result would be declared by the galvanometer. The quantity absorbed by the oxygen under those circumstances was too feeble to affect the galvanometer; the gas, in fact, proved sensibly transparent to the rays of heat. It had but a feeble power of radiation: it had an equally feeble power of absorption.

The pile remaining in its position, a sheet of olefiant gas was caused to issue from the same slit as that through which the oxygen had passed. No one present could see the gas; it was quite invisible, the light went through it as freely as through oxygen or air; but its effect upon the thermal rays emanating from the cube, was what might be expected from a sheet of metal. A quantity so large was cut off, that the needle of the galvanometer, promptly quitting the zero line, moved with energy to its stops: thus the olefiant gas, so light and clear and pervious to luminous rays, was a most potent destroyer of the rays emanating from an obscure source. The reciprocity of action established in the case of oxygen comes out here; the good radiator is found by this experiment to be the good absorber.

This result, which was exhibited before a public audience this evening for the first time, was typical of what had been obtained with gases generally. Going through the entire list of gases and vapours in this way, we should find radiation and absorption to be as rigidly associated as positive and negative in electricity, or as north and south polarity in magnetism. The gas which, when heated, is most competent to generate a calorific ray, is precisely that which is most competent to stop such a ray. If the radiation be high, the absorption is high; if the radiation be moderate, the absorption is moderate; if the radiation be low, the absorption is low; so that if we make the number which expresses the absorptive power the numerator of a fraction, and that which expresses its radiative power, the denominator, the result would be, that, on account of the numerator and denominator varying in the same proportion, the value of that fraction would always remain the same, whatever might be the gas or vapour experimented with.

But why should this reciprocity exist? What is the meaning of absorption? what is the meaning of radiation? When you cast a stone into still water, rings of waves surround the place where it falls; motion is radiated on all sides from the centre of disturbance. When the hammer strikes a bell, the latter vibrates; and sound, which is nothing more than an undulatory motion of the air, is radiated in all directions. Modern philosophy reduces light and heat to the same mechanical category. A luminous body is one with its particles in a state of vibration; a hot body is one with its particles also vibrating, but at a rate which is incompetent to excite the sense of vision; and, as a sounding body has the air around it, through which it propagates its vibrations, so also the luminous or heated body has a medium, called ether, which accepts its motions and carries them forward with inconceivable velocity. Radiation, then, as regards both light and heat, is *the transference of motion from the vibrating body to the ether in which it swings*; and, as in the case of sound, the motion imparted to the air is soon transferred to the surrounding objects, against which the aerial undulations strike, the sound being, in technical language, *absorbed*; so also with regard to light and heat, absorption consists in *the transference of motion from the agitated ether to the particles of the absorbing body*.

The simple atoms are found to be bad radiators; the compound atoms good ones: and the higher the degree of complexity in the atomic grouping, the more potent, as a general rule, is the radiation and absorption. Let us get definite ideas here, however gross, and purify them afterwards by the process of abstraction. Imagine our simple atoms swinging like single spheres in the ether; they cannot create the swell which a group of them united to form a system can produce. An oar runs freely edgeways through the water, and imparts far less of its motion to the water than when its broad flat side is brought to bear upon it. In our present language the oar, broad side vertical, is a good radiator; broad side horizontal, it is a bad radiator. Conversely the waves of water, impinging upon the flat face of the oar-blade, will impart a greater amount of motion to it than when impinging upon the edge. In the position in which the oar radiates well, it also absorbs well. Simple atoms glide through the ether without much resistance; compound ones encounter this, and yield up more speedily their motion to the ether. *Mix* oxygen and nitrogen mechanically, they absorb and radiate a certain amount. Cause these gases to *combine* chemically and form nitrous oxide, both the absorption and radiation are thereby augmented 250 times!

In this way we look with the telescope of the intellect into atomic systems, and obtain a conception of processes which the eye of sense can never reach. But gases and vapours possess a power of choice as to the rays which they absorb. They single out certain groups of rays for destruction, and allow other groups to pass unharmed. This is best illustrated by a famous experiment of Sir David Brewster's, modified to suit the requirements of the present discourse. Into a

glass cylinder, with its ends stopped by discs of plate-glass, a small quantity of nitrous acid gas was introduced; the presence of the gas being indicated by its rich brown colour. The beam from an electric lamp being sent through two prisms of bisulphide of carbon, a spectrum seven feet long and eighteen inches wide was cast upon a screen. Introducing the cylinder containing the nitrous acid into the path of the beam as it issued from the lamp, the splendid and continuous spectrum became instantly furrowed by numerous dark bands, the rays answering to which were struck down by the nitric gas, while it permitted the light which fell upon the intervening spaces to pass with comparative impunity.

Here also the principle of reciprocity, as regards radiation and absorption, holds good; and could we, without otherwise altering its physical character, render that nitrous gas luminous, we should find that the very rays which it absorbs are precisely those which it would emit. When atmospheric air and other gases are brought to a state of intense incandescence by the passage of an electric spark, the spectra which we obtain from them consist of a series of bright bands. But such spectra are produced with the greatest brilliancy, when, instead of ordinary gases, we make use of metals heated so highly as to volatilize them. This is easily done by the voltaic current. A capsule of carbon was filled with mercury, which formed the positive electrode of the electric lamp; a carbon point was brought down upon this; and on separating one from the other, a brilliant arc containing the mercury in a volatilized condition passed between them. The spectrum of this arc was not continuous like that from the solid carbon points, but consisted of a series of vivid bands, each corresponding in colour to that particular portion of the spectrum to which its rays belonged. Copper gave its system of bands; zinc gave its system; and brass, which is an alloy of copper and zinc, gave a splendid spectrum made up of the bands belonging to both metals.

Not only, however, when metals are united like zinc and copper to form an alloy, is it possible to obtain the bands which belonged to them. No matter how we may disguise the metal—allowing it to unite with oxygen to form an oxide, and this again with an acid to form a salt; if the heat applied be sufficiently intense, the bands belonging to the metal reveal themselves with perfect definition. Holes were drilled in a cylinder of retort carbon, and these being filled with pure culinary salt, the carbon was made the positive electrode of the lamp: the resultant spectrum showed the brilliant yellow lines of the metal sodium. Similar experiments were made with the chlorides of strontium, calcium, lithium,* and other metals; each salt gave the bands due to the metal. Different salts were then mixed together, and rammed into the holes in the carbon; a spectrum was obtained which contained the bands of them all.

* The vividness of the colours of the lithium spectrum is extraordinary: it contained a blue band of indescribable splendour. It was thought by many,

The position of these bright bands never varies, and each metal has its own system. Hence the competent observer can infer from the bands of the spectrum the metals which produce it. It is a language addressed to the eye instead of the ear; and the certainty would not be augmented if each metal possessed the power of audibly calling out, "I am here!" Nor is this language affected by distance. If we find that the sun or the stars give us the bands of our terrestrial metals, it is a declaration on the part of these orbs that such metals enter into their composition. Does the sun give us any such intimation? Does the solar spectrum exhibit bright lines which we might compare with those produced by our terrestrial metals, and prove either their identity or difference? No. The solar spectrum, when closely examined, gives us a multitude of fine dark lines instead of bright ones. They were first noticed by Dr. Wollaston, were investigated with profound skill by Fraunhofer, and named from him Fraunhofer's lines. They have been long a standing puzzle to philosophers. The bright lines which the metals give us have been also known to us for years; but the connection between both classes of phenomena was wholly unknown, until Kirchhoff, with admirable acuteness, revealed the secret, and placed it at the same time in our power to chemically analyze the sun.

We have now some hard work before us; hitherto we have been delighted by objects which addressed themselves rather to our æsthetic taste than to our scientific faculty. We have ridden pleasantly to the base of the final cone of Etna, and must now dismount and march wearily through ashes and lava, if we would enjoy the prospect from the summit. Our problem is to connect the dark lines of Fraunhofer with the bright ones of the metals. The white beam of the lamp is refracted in passing through our two prisms, but its different components are refracted in different degrees, and thus its colours are drawn apart. Now the colour depends solely upon the rate of oscillation of the particles of the luminous body; red light being produced by one rate, blue light by a much quicker rate, and the colours between red and blue by the intermediate rates. The solid incandescent coal-points give us a continuous spectrum; or in other words they emit rays of all possible periods between the two extremes of the spectrum. They have particles oscillating so as to produce red; others, to produce orange; others, to produce yellow, green, blue, indigo, and violet respectively. Colour, as many of you know, is to light what *pitch* is to sound. When a violin-player presses his finger on a string he makes it shorter and tighter, and thus, causing it to vibrate more speedily, augments the pitch. Imagine such a player to move his finger slowly along the string, shortening it gradually as he draws his bow, the note would rise in pitch by a regular gradation; there would be no gap intervening

during the discourse, that I had mistaken strontium for lithium, as this blue band had never before been seen. I have obtained it many times since; and my friend Dr. Miller, having kindly analyzed the substance made use of, pronounces it chloride of lithium.—J. T.

between note and note. Here we have the analogue to the continuous spectrum, whose colours insensibly blend together without gap or interruption, from the red of the lowest pitch to the violet of the highest. But suppose the player, instead of gradually shortening his string, to press his finger on a certain point, and to sound the corresponding note; then to pass on to another point more or less distant, and sound its note; then to another, and so on, thus sounding particular notes separated from each other by gaps which correspond to the intervals of the string passed over; we should then have the exact analogue of a spectrum composed of separate bright bands with intervals of darkness between them. But this, though a perfectly true and intelligible analogy, is not sufficient for our purpose; we must look with the mind's eye at the very oscillating atoms of the volatilized metal. Figure these atoms connected by springs of a certain tension, and which, if the atoms are squeezed, together push them asunder, or if the atoms are drawn apart, pull them together, causing them, before coming to rest, to quiver at a certain definite rate determined by the strength of the spring. Now the volatilized metal which gives us one bright band is to be figured as having its atoms united by springs all of the same tension, its vibrations are all of one kind. The metal which gives us two bands may be figured as having some of its atoms united by springs of one tension, and others by a second series of springs of a different tension. Its vibrations are of two distinct kinds; so also when we have three or more bands, we are to figure as many distinct sets of springs, each set capable of vibrating in its own particular time and at a different rate from the other. If we seize this idea definitely, we shall have no difficulty in dropping the metaphor of springs, and substituting for it mentally the forces by which the atoms act upon each other. Having thus far cleared our way, let us make another effort to advance.

Here is a pendulum,—a heavy ivory ball suspended from a string. I blow against this ball; a single puff of my breath moves it a little way from its position of rest; it swings back towards me, and when it reaches the limit of its swing I puff again. It now swings further; and thus by timing my puffs I can so accumulate their action as to produce oscillations of large amplitude. The ivory ball here has absorbed the motions which my breath communicated to the air. I now bring the ball to rest. Suppose, instead of my breath, a wave of air to strike against it, and that this wave is followed by a series of others which succeed each other exactly in the same intervals as my puffs; it is perfectly manifest that these waves would communicate their motion to the ball and cause it to swing as the puffs did. And it is equally manifest that this would not be the case if the impulses of the waves were not properly timed; for then the motion imparted to the pendulum by one wave would be neutralized by another, and there could not be that accumulation of effect which we have when the periods of the waves correspond with the periods of the pendulum. So much for the kind of impulses absorbed by the pendulum. But such a pendulum set oscillating in air produces waves in the air; and we

see that the waves which it produces must be of the same period as those whose motions it would take up or absorb most copiously if they struck against it. Just in passing I may remark, that if the periods of the waves be double, treble, quadruple, &c., the periods of the pendulum, the shocks imparted to the latter would also be so timed as to produce an accumulation of motion.

Perhaps the most curious effect of these timed impulses ever described was that observed by a watchmaker, named Ellicott, in the year 1741. He set two clocks leaning against the same rail; one of them, which we may call A, was set going; the other, B, not. Some time afterwards he found, to his surprise, that B was ticking also. The pendulums being of the same length the shocks imparted by the ticking of A to the rail against which both clocks rested were propagated to B, and were so timed as to set B going. Other curious effects were at the same time observed. When the pendulums differed from each other a certain amount, A set B going, but the re-action of B stopped A. Then B set A going, and the re-action of A stopped B. If the periods of oscillation were close to each other, but still not quite alike, the clocks mutually controlled each other, and by a kind of mutual compromise they ticked in perfect unison.

But what has all this to do with our present subject? They are mechanically identical. The varied actions of the universe are all modes of motion; and the vibration of a ray claims strict brotherhood with the vibrations of our pendulum. Suppose ethereal waves striking upon atoms which oscillate in the same periods as the waves succeed each other, the motion of the waves will be absorbed by the atoms; suppose we send our beam of white light through a sodium flame, the particles of that flame will be chiefly affected by those undulations which are synchronous with their own periods of vibration. There will be on the part of those particular rays a transference of motion from the agitated ether to the atoms of the volatilized sodium, which, as already defined, is absorption. We use glass screens to defend us from the heat of our fires; how do they act? Thus:—The heat emanating from the fire is for the most part due to radiations which are incompetent to excite the sense of vision; we call these rays obscure. Glass, though pervious to the luminous rays, is opaque in a high degree to those obscure rays, and cuts them off, while the cheerful light of the fire is allowed to pass. Now mark me clearly. The heat cut off from your person is to be found in the glass, the latter becomes heated and radiates towards your person; what then is the use of the glass if it merely thus acts as a temporary halting-place for the rays, and sends them on afterwards. It does this:—It not only sends the heat it receives towards you, but scatters it also in all other directions round the room. Thus the rays which, were the glass not interposed, would be shot directly against your person, are for the most part diverted from their original direction, and you are preserved from their impact.

Now for our experiment. I pass the beam from the electric lamp through the two prisms, and the spectrum spreads its colours upon the

screen. Between the lamp and the prism I interpose this snapdragon light. Alcohol and water are here mixed up with a quantity of common salt, and the metal dish that contains them is heated by a spirit-lamp. The vapour from the mixture ignites and we have this monochromatic flame. Through this flame the beam from the lamp is now passing; and observe the result upon the spectrum. You see a dark band cut out of the yellow,—not very dark, but sufficiently so to be seen by everybody present. Observe how the band quivers and varies in shade as the amount of yellow light cut off by the unsteady flame varies in amount. The flame of this monochromatic lamp is at the present moment casting its proper yellow light upon that shaded line; and more than this, it casts, in part, the light which it absorbs from the electric lamp upon it; but it scatters the greater portion of this light in other directions, and thus withdraws it from its place upon the screen, as the glass, in the case above supposed, diverted the heat of the fire from your person. Hence the band appears dark; not absolutely, but dark in comparison with the adjacent brilliant portions of the spectrum.

But let me exalt this effect. I place in front of the electric lamp the intense flame of a large Bunsen's burner. I have here a platinum capsule into which I put a bit of sodium less than a pea in magnitude. The sodium placed in the flame soon volatilizes and burns with brilliant incandescence. Observe the spectrum. The yellow band is clearly and sharply cut out, and a band of intense obscurity occupies its place. I withdraw the sodium, the brilliant yellow of the spectrum takes its proper place: I reintroduce the sodium and the black band appears.

Let me be more precise:—The yellow colour of the spectrum extends over a sensible space, blending on one side into orange and on the other into green. The term "yellow band" is therefore somewhat indefinite. I want to show you that it is the precise yellow band emitted by the volatilized sodium which the same substance absorbs. By dipping the coal-point used for the positive electrode into a solution of common salt, and replacing it in the lamp, I obtain that bright yellow band which you now see drawn across the spectrum. Observe the fate of that band when I interpose my sodium light. It is first obliterated, and instantly that black streak occupies its place. See how it alternately flashes and vanishes as I withdraw and introduce the sodium flame!

And supposing that instead of the flame of sodium alone, I introduce into the path of the beam a flame in which lithium, strontium, magnesium, calcium, &c., are in a state of volatilization, each metallic vapour would cut out its own system of bands, each corresponding exactly in position with the bright band which that metal itself would cast upon the screen. The light of our electric lamp then shining through such a composite flame would give us a spectrum cut up by dark lines, exactly as the solar spectrum is cut up by the lines of Fraunhofer.

And hence we infer the constitution of the great centre of our system. The sun consists of a nucleus which is surrounded by a flaming atmosphere. The light of the nucleus would give us a continuous spectrum, as our common coal-points did ; but having to pass through the photosphere, as our beam through the flame, those rays of the nucleus which the photosphere can itself emit are absorbed, and shaded spaces, corresponding to the particular rays absorbed, occur in the spectrum. Abolish the solar nucleus, and we should have a spectrum showing a bright band in the place of every dark line of Fraunhofer. These lines are therefore not absolutely dark, but dark by an amount corresponding to the difference between the light of the nucleus intercepted by the photosphere, and the light which issues from the latter.

The man to whom we owe this beautiful generalization is Kirchhoff, Professor of Natural Philosophy in the university of Heidelberg ; but, like every other great discovery, it is compounded of various elements. Mr. Talbot observed the bright lines in the spectra of coloured flames. Sixteen years ago Dr. Miller gave drawings and descriptions of the spectra of various coloured flames. Wheatstone, with his accustomed ingenuity, analyzed the light of the electric spark, and showed that the metals between which the spark passed determined the bright bands in the spectrum of the spark. Masson published a prize essay on these bands ; Van der Willigen, and more recently Plücker, have given us beautiful drawings of the spectra, obtained from the discharge of Ruhmkorff's coil. But none of these distinguished men betrayed the least knowledge of the connection between the bright bands of the metals and the dark lines of the solar spectrum. The man who came nearest to the philosophy of the subject, was Ångström. In a paper translated from Poggendorff's "Annalen" by myself, and published in the "Philosophical Magazine" for 1855, he indicates that the rays which a body absorbs are precisely those which it can emit when rendered luminous. In another place, he speaks of one of his spectra giving the general impression of *reversal* of the solar spectrum. Foucault, Stokes, and Thomson, have all been very close to the discovery ; and, for my own part, the examination of the radiation and absorption of heat by gases and vapours, some of the results of which I placed before you at the commencement of this discourse, would have led me in 1859 to the law on which all Kirchhoff's speculations are founded, had not an accident withdrawn me from the investigation. But Kirchhoff's claims are unaffected by these circumstances. True, much that I have referred to formed the necessary basis of his discovery ; so did the laws of Kepler furnish to Newton the basis of the theory of gravitation. But what Kirchhoff has done carries us far beyond all that had before been accomplished. He has introduced the order of law amid a vast assemblage of empirical observations, and has ennobled our previous knowledge by showing its relationship to some of the most sublime of natural phenomena.

[J. T.]

Friday, January 17, 1862.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. Vice-President,
in the Chair.

JOHN TYNDALL, F.R.S. &c. &c.

PROFESSOR OF NATURAL PHILOSOPHY AT THE ROYAL INSTITUTION.

On the Absorption and Radiation of Heat by Gaseous Matter.

RESUMING with a new apparatus his experiments on the influence of Chemical Combination on the Absorption and Radiation of Heat by Gases, the speaker, in the investigation of which the evening's discourse would be a *résumé*, first examines the deportment of chlorine as compared with hydrochloric acid, and of bromine as compared with hydrobromic acid, and finds that the act of combination which in each of these two cases notably diminishes the density of the gas and renders the coloured gas perfectly transparent to light, renders it more opaque for obscure heat. He also draws attention to the fact that sulphur, which is partially opaque to light, is transparent to 54 per cent. of the rays issuing from a source of 100 C. while its compound, heavy spar, which is sensibly transparent to light, is quite opaque to the rays from a source of 100 C. He demonstrates, in confirmation of Melloni, the transparency of lampblack in thin layers; but shows how irreconcilable its deportment to radiant heat is with the idea generally prevalent at the present day, that lampblack absorbs heat of all kinds with the same intensity.

All his experiments with gases have been repeated with a different source of heat, and he finds the result still more pronounced than formerly, that the compound gases far transcend the elementary ones in absorptive power. Taking air as unity, ammonia, at 30 inches tension, is 1195, this latter figure representing *all the heat* that issued from the source. A layer of ammonia, 3 feet long, is *perfectly black* to heat emanating from an obscure source. The coloured gases, chlorine and bromine, though much superior in absorptive power to the transparent elementary gases, are exceeded in this respect by every compound gas that has been hitherto examined. When, instead of tensions of 30 inches, we compare tensions of 1 inch, the differences between the gases come out still more strikingly. At this tension, for example, the absorption of sulphurous acid is eight thousand times that of air.

The speaker also referred to a new and extensive series of experiments on the Absorption of Radiant Heat by Vapours. The least energetic, as before, he finds to be bisulphide of carbon; the most

energetic, boracic ether. He shows that the absorption of the latter vapour (which is quite transparent) at 0.1 of an inch of tension is 600 times the absorption of the densely coloured vapour of bromine, while in all probability it is 186,000 times that of air.

The speaker was led by a series of perplexing experiments, which are fully described in a Memoir recently presented to the Royal Society, to the solution of the following remarkable and at first sight utterly paradoxical problem—“*To determine the absorption and radiation of a gas or vapour without any source of heat external to the gaseous body itself.*”

When air enters a vacuum it is heated by the stoppage of its motion ; when a vessel containing air is exhausted by an air-pump, chilling is produced by the application of a portion of the heat of the air to generate *vis viva*. Let us call the heating in the first case dynamic heating, and the chilling in the second case dynamic chilling. Let us further call the radiation of a gas which has been heated dynamically, dynamic radiation, and the absorption of a gas which has been chilled dynamically, dynamic absorption. Placing a thermo-electric pile at the end of his experimental tube, the latter being exhausted, the gas to be examined is permitted to enter the tube ; the gas is heated, and if it possess any sensible radiative power, the pile will receive its radiation, and the galvanometer connected with the pile will declare it.

Proceeding in this way with gases, Professor Tyndall found that the radiation thus manifested, and which was sometimes so intense as to urge the needle of the galvanometer through an arc of more than sixty degrees, followed the exact order of the absorptions which he had already determined. After the heat of the radiating column of gas had wasted itself, the air-pump was worked at a certain rate, the rarefied gas within the tube became chilled, and the face of the pile turned towards the chilled gas became correspondingly lowered in temperature. The dynamic absorptions of various gases were thus determined, and they were found to go strictly hand in hand with the dynamic radiation.

In the case of vapours the following method was pursued. A quantity of the vapour sufficient to depress the mercury column 0.5 of an inch was admitted into the tube, and this was heated dynamically by allowing dry air to enter till the tube was filled. The radiation of the vapours thus determined followed exactly the same order as the absorption which had already been measured. The dynamic absorption of the vapour was obtained by pumping out in the manner just described, and it was found to follow the same order as the dynamic radiation. In these experiments the air bore the same relationship to the vapour that a polished silver surface does to a coat of varnish laid over it. Neither the silver nor the air, both of which are elements or mixtures of elements, possesses the power of agitating in any marked degree the luminiferous ether. But the motion of the silver being communicated to the varnish, and the motion of the air being commu-

nicated to the vapour, molecules are agitated which have the power of disturbing, in a very considerable degree, the ether in which they swing.

The speaker finds by strict experiments that the dynamic radiation of an amount of boracic ether vapour, possessing a tension of only $\frac{1}{1000000}$ th of an atmosphere is easily measurable. He also shows and explains the fact that with a tube 33 inches long, the dynamic radiation of acetic ether considerably exceeds that of olefiant gas ; while in a tube 3 inches long, the dynamic radiation of olefiant gas considerably exceeds that of the ether. Aqueous vapour has been subjected to a special examination, and Professor Tyndall finds it a common fact for the aqueous vapour contained in the atmosphere to exercise 60 times the absorption of the air itself. The further he has pursued his attempts to obtain perfectly pure and dry air, the more has the air approached the character of a vacuum. He further points to the possibility of determining the temperature of space by direct experiment.

Scents of various kinds have been examined. Dry air was passed over bibulous paper moistened by the essential oils, and carried into the experimental tube. Small as the amount of matter here entering the tube is known to be, it was found that the absorption of radiant heat by those odours varies from 30 times to 372 times that of the air which formed the vehicle. The speaker remarked that the absorption of terrestrial rays by the odour of a flower-bed may exceed in amount that of the entire oxygen and nitrogen of the atmosphere above the bed.

Ozone has also been subjected to examination. The substance was obtained by the electrolysis of water, and from decomposing cells containing electrodes of various sizes. Calling the action of the ordinary oxygen, which entered the experimental tube with the ozone unity, the absorption of the ozone itself was in six different experiments,—21, 36, 47, 65, 85, 136. The augmenting action of the ozone accompanied the *diminution of the size of the electrodes* used in the decomposing cells. Professor Tyndall points out the perfect correspondence of these last results with those of M. Meidinger by a totally different method of experiment.

[J. T.]

Friday, February 14, 1862.

REV. JOHN BARLOW, M.A. F.R.S. Vice-President, in the Chair.

WILLIAM ODLING, M.B. F.R.S.

SEC. CHEM. SOC.

On Mr. Graham's Researches on Dialysis.

Vial Diffusion.—When an open vial, filled with a solution of some salt or other substance, is introduced into a jar of water, a portion of the dissolved salt passes gradually from the vial into the external water. This portion is known as the *diffusate*. The diffusates yielded by different substances under precisely the same circumstances vary greatly. Thus, common salt yields double the amount of diffusate that Epsom salt yields, while Epsom salt diffuses twice as rapidly as gum-arabic. Every substance has its own definite rate of diffusibility in the same liquid medium, dependent upon the nature of the medium,—whether water or alcohol, for instance. The diffusion of any particular salt is scarcely affected by the presence of a different salt, though materially affected by the presence of the same salt in the external liquid. The general law seems to be, that the velocity with which a dissolved salt diffuses from a stronger into a weaker solution is proportional to the difference of concentration, *quoad* that salt, between the contiguous solutions. The phenomena of liquid diffusion are manifested most simply and uniformly when dilute solutions only are employed.

Jar Diffusion.—When the solution of the salt to be diffused, instead of being placed in a vial, is conveyed by means of a pipette to the bottom of a jar of water, the dissolved salt gradually rises through the superincumbent water to a height or extent proportional to its diffusibility. The results of jar diffusion bear out generally those of vial diffusion. They show, moreover, the absolute rate or velocity of the diffusive movement. Thus, during a fourteen days' aqueous diffusion from ten per cent. solutions of gum-arabic, Epsom salt, and common salt respectively, the gum-arabic rose only through $\frac{1}{4}$ ths of the superincumbent water, or to a height of 55.5 millimetres; while the Epsom salt rose through the whole $\frac{1}{4}$ ths of the superincumbent water, or to a height of 111 millimetres; while the common salt not only rose to the top, but could have risen much higher, seeing that the uppermost or 14th stratum of water into which it had diffused, contained about fifteen times as much salt as was contained in the upper-

most or 14th stratum of water into which the Epsom salt had diffused.

DIFFUSIVE SEPARATIONS.—If a solution containing equal weights of common salt and gum arabic be poured into a jar of water, the ratio of salt to gum in the dilute liquid in the jar will be the same as in the original solution. But if the original solution be poured into a diffusion vial from which the dissolved compounds can diffuse into a jar of water, for every 100 milligrammes of salt, about $22\frac{1}{2}$ milligrammes of gum will pass out into the external water, or the ratio of salt to gum in the dilute liquid in the jar will be as 100 to $22\frac{1}{2}$, instead of as 100 to 100, or a separation of the gum from the salt to the extent of $77\frac{1}{2}$ per cent. will have been effected. Again, when a solution containing 5 per cent. of common salt and 5 per cent. Glauber's salt (the diffusive rates of which salts are to one another as 100 to 70) is submitted for seven days to the process of jar diffusion, the upper half, or $\frac{1}{2}$ ths of superincumbent water, will be found to contain 380 milligrammes of common salt and only 53 milligrammes of Glauber's salt, or the ratio of common salt to Glauber's salt on the upper half of the liquid will be as 100 to 14, instead of as 100 to 100, or a separation of the Glauber's salt from the common salt to the extent of 86 per cent. will have been effected. Not only the partial separation of mixed bodies, but the partial decomposition of definite chemical compounds may be effected by diffusion. Thus when alum, which is a double sulphate of the two metals, potassium and aluminium, is allowed to diffuse from its aqueous solution, the diffusive tendency of potassium compounds being much greater than that of aluminium compounds, a portion of sulphate of potassium actually breaks away from the sulphate of aluminium with which it was in combination, in order to diffuse into the external or superincumbent water more freely than the sulphate of aluminium can diffuse.

Crystalloids and Colloids.—Diffusibility does not seem to be associated in any definite way with chemical composition. Thus, complex organic bodies, like picric acid and sugar, have much the same diffusion-rates as common salt and Epsom salt respectively. Isomorphous compounds, however, are for the most part equi-diffusive, but the groups of equi-diffusive are larger than those of isomorphous bodies. The common salt group, for instance, includes not only chloride, bromide, and iodide of sodium, which are similar in composition and isomorphous in form, but also nitrate of sodium, which is dissimilar in composition and heteromorphous in form. But in comparing highly diffusive substances on the one hand, with feebly diffusive substances on the other, one broad dissimilarity becomes apparent, namely, that highly diffusive substances affect the crystalline state, while feebly diffusive substances are amorphous, and, in particular, are characterized by a capability of forming gelatinous hydrates. Hence the distinction established by Mr. Graham between highly diffusive bodies, or *crystalloids*, and feebly diffusive bodies, or *colloids*. There are very many compounds which can exist, both in the crystalline and gelatinous states, and

which present two distinct diffusion-rates corresponding respectively thereto.

Nature of Diffusion.—Liquid diffusion may be attributed to a self-repulsion of the particles of the salt or other body which diffuses on the one hand, and of the particles of water or other liquid into which it diffuses on the other; or it may be attributed to a particular kind of mutual attraction existing between the particles of the salt and of the water. Assuming the truth of this latter hypothesis, it is clear that the diffusive attraction of crystalloid particles for water is greater than that of colloid particles; and, recognizing this superior diffusive attraction of saline particles, it is conceivable that a salt should be able to unite diffusively, not only with free water, but also with water that is already in a low form of combination, as it exists in a soft solid, for instance, such as jelly: and this is found to be the case. Common salt, for example, diffuses into a mass of solid jelly almost as easily and extensively as into a similar bulk of free water. But although the introduction of a gelatinous substance does not interfere in any appreciable way with the diffusion of a crystalloid, it arrests almost completely the diffusion of a colloid body. The colloid has but very little tendency to unite diffusively, even with free water, and is quite incapable of abstracting and uniting diffusively with water that is in any state of combination, however feeble.

Dialysis.—Although a simple diffusion into water can effect the partial separation of a highly diffusible from a feebly diffusible substance, yet a much better result may be obtained by causing the diffusion to take place, not into free water, but into or through the combined water of a soft solid, which, as before observed, scarcely affects the diffusion of crystalloid, but almost arrests that of a colloid body. When a piece of vellum, or of membrane, or of parchment-paper, or even a layer of mucus, is interposed between a colloid solution and a quantity of water, the colloid, in order to get to the free water, must pass through the membrane, that is to say, must unite diffusively with the combined water of the membrane, which however it is incapable of doing. The crystalloid body can pass through, but the colloid cannot. This constitutes *dialysis*, which means the separation of crystalloid from colloid bodies, through a membrane that will allow the passage of crystalloid, but will not allow that of colloid particles; because the crystalloid particles, having a highly diffusive power, can unite diffusively with the combined water of the soft solid septum, so as to reach the external free water; whereas the colloid particles, having a very feebly diffusive power, cannot. The process of dialysis is altogether different from that of filtration. Filtration refers to the passage of masses through appreciable pores, but in a dialytic septum there are no pores, and the movement is not molar but molecular. The dialytic septum will allow chemical action to take place, or that low form of chemical action which constitutes diffusion; but, being quite impervious to the mechanical passage of liquid, it will not allow of filtration.

Colloid Solutions.—The solution of various colloids has been heretofore effected by means of crystalloid chemicals, comprising acids, bases, and salts. By dialyzing these liquids, the crystalloid reagents diffuse away, and leave the colloid bodies in simple aqueous solution. Mr. Graham has thus obtained pure colloidal solutions in water of numerous mineral and organic substances, such, for instance, as silica, tin-stone, alumina, hæmatite, chrome, Prussian blue, prussiate of copper, sucrate of copper and other sucrares, tannin, gum, caramel, albumin, &c. These colloidal solutions are for the most part unstable. Either spontaneously, or on the addition of a very minute quantity of some or other crystalloid reagent, they pectize, or become converted into solid jellies. Hence Mr. Graham speaks of two colloidal states, the *peptous*, or dissolved, and the *pectous* or gelatinized. Colloid bodies are characterized by their non-crystalline habit, by their low diffusibility, by their chemical inertness, by their high atomicity, and above all by their mutability. All these properties are exceedingly well manifested by colloid silica, or co-silicic acid.

[W. O.]

Friday, April 11, 1862.

JOHN PETER GASSIOT, Esq. F.R.S. Vice-President, in the Chair.

Dr. A. W. HOFMANN, F.R.S. Pres. C.S.

On Mauve and Magenta.

THE fact of the beautiful colouring matters known by these fanciful terms being substances derived from coal, must, I presume, be familiar to every one of you. But there may be many unacquainted with the means by which this transformation is accomplished. It is to them that I address myself this evening.

Coal to become colour, has to pass through a series of stages of transition, each of which claims our attention for a moment. Briefly expressed, the aim of this address may be said to be, to show you the way from *coal* to *colour*. Now let me at once tell you this way is rather long; we may have to travel over country rough and intricate, and now and then to pass through territories which—I confess it with an appeal to the ladies—may not, perhaps, be altogether remarkable for their fragrance and sweetness. But on such occasions we shall accelerate our steps, and, on the whole, I venture to hope that we shall arrive at our journey's end without too much inconvenience.

Colour is intimately associated with light; without light there is no colour. This remark applies in a double sense to the colours derived from coal; for it is to the introduction of gas-light for illuminating our streets and houses, that we are indebted for the acquisition of these colours. This statement may appear strange, for nearly half a century has elapsed during which we have been in the possession of gas, whilst the transformation of coal into colouring matters has been achieved only recently under our own eyes. But you will immediately appreciate the truth of my assertion, if I tell you that these substances are obtained from a secondary product, generated in the manufacture of gas, a product long used for a variety of purposes, but which, only within the last few years, the researches of chemists have proved to be an inexhaustible mine of wealth and interest.

The starting-point then for the production of Mauve and Magenta,

is the manufacture of coal-gas; but this is so well known as not to need any detailed description. Let me briefly remind you of the principal features of the distillation of coal, by directing your attention to the two large diagrams representing the "retort house" and the "condensers" of a gas-work. You observe how the coal is heated in stupendous retorts, five or seven of which are generally associated in one furnace. The gas ascends from these retorts in vertical tubes, the bent ends of which dip into a large horizontal pipe, partly filled with water, called the *hydraulic main*, a considerable amount of the oily and tarry substances generated with the gas being separated by the water. The gas, so far purified, passes on through the condensers—immense vertical iron pipes constantly cooled by a current of cold water which surrounds their external surface. In these condensers an additional quantity of oily matter is separated, which, together with the oily substances deposited by the gas during its passage through the hydraulic main, is collected in appropriately placed cisterns. The gas, having traversed the condensers, passes through a series of further purifications before it is delivered into the mains of our streets; but these, unconnected as they are with our subject, must no longer occupy our attention.

The distillation of coal being the fundamental operation in the manufacture of Mauve and Magenta, it is but fair that it should not remain without an experimental illustration. In this tubulated retort of hard glass, I am heating fragments of coal. The beak of the retort is inserted into a three-necked glass globe, the lower neck of which terminates in a tube, communicating with a glass vessel for the collection of the "oily products;" while the third neck is provided with a delivery-tube for the discharge of the gas, which finds its way into a glass gas-holder. The coal has been heated only for a few minutes, and the gas already begins to be freely evolved; already I may light it at the orifice of the delivery-tube, which, for this purpose, I have removed from the gas-holder; already it burns with the characteristic luminous flame of coal-gas. In the meanwhile, you observe, a considerable quantity of the "oily products" has accumulated in the receiver. Their formation continues as long as the gas is evolved. Ultimately the coal is entirely resolved into *gas* and *oily products*, a non-volatile residue, the *coke*, remaining behind in the retort.

It is in the oily products, the so-called "*coal-tar-oil*," that our interest is centred. To my mind this coal-tar-oil * is one of the most wonderful productions in the whole range of chemistry. That may be rather a one-sided view, but having in younger years spent much time in the investigation of this substance, I have acquired quite an affection for it. Nor can you fail to appreciate the interest which coal-tar presents to the chemist when you look at the diagram in which I have endeavoured to arrange synoptically the various substances which have been eliminated from it.

* A large specimen of coal-tar-oil was here exhibited.

PRODUCTS OF THE DESTRUCTIVE DISTILLATION OF COAL.

Name.	Formula.	Boiling Points.
Hydrogen	H H	
Marsh gas, or Hydride of methyl .	C H ₃ , H	
Hydride of Hexyl	C ₆ H ₁₃ , H	
Hydride of Octyl	C ₈ H ₁₇ , H	
Hydride of Decyl	C ₁₀ H ₂₁ , H	
Olefiant gas, or Ethylene . . .	C ₂ H ₄	
Propylene, or Tetrylene . . .	C ₃ H ₆	
Caproylene, or Hexylene . . .	C ₆ H ₁₂ . .	55°
Cenanthylene, or Heptylene . .	C ₇ H ₁₄	
Paraffin	C _n H _{2n} (?)	
Acetylene	C ₂ H ₂	
Benzol	C ₆ H ₆ . .	84°
Parabenzol	C ₆ H ₆	
Toluol	C ₇ H ₈ . .	114°
Xylol	C ₈ H ₁₀ . .	126°
Cumol	C ₉ H ₁₂ . .	150°
Cymol	C ₁₀ H ₁₄ . .	175°
Naphtalin	C ₁₀ H ₈ . .	212°
Paranaphtalin, or Anthracen . .	C ₁₄ H ₁₀	
Chrysen	C ₁₂ H ₈ (?)	
Pyren	C ₂₀ H ₁₄	
Eupion	(?)	
Water	H } O . .	100°
Hydrosulphuric acid	H } S	
Hydrosulphocyanic acid	H } S	
	(C N) }	
Carbonic oxide	C O	
Carbonic anhydride	C O ₂	
Disulphide of carbon	C S ₂	47°
Sulphurous anhydride	S O ₂	
Acetic acid	(C ₂ H ₃ O) } O	120°
Phenyllic acid, or alcohol, <i>Phenol</i>	(C ₆ H ₅) } O	188°

PRODUCTS OF THE DESTRUCTIVE DISTILLATION OF COAL—continued.

Name.	Formula.	Boiling Points.
Cresylic acid, or alcohol, Cresol	$(C_7 H_7) \left\{ \begin{smallmatrix} H \\ H \end{smallmatrix} \right\} O$	203°
Phlorylic acid, or alcohol, Phlorol	$(C_8 H_9) \left\{ \begin{smallmatrix} H \\ H \end{smallmatrix} \right\} O$	
Rosolic acid		
Brunolic acid	(?)	
Ammonia	$\left\{ \begin{smallmatrix} H \\ H \\ H \end{smallmatrix} \right\} N$	
Aniline	$C_6 H_5 \left\{ \begin{smallmatrix} H \\ H \\ H \end{smallmatrix} \right\} N$	182°
Cespitine	$(C_5 H_{18})''' N$	96°
Pyridine	$(C_5 H_5)''' N$	115°
Picoline	$(C_6 H_7)''' N$	134°
Lutidine	$(C_7 H_9)''' N$	154°
Collidine	$(C_8 H_{11})''' N$	170°
Parvoline	$(C_9 H_{13})''' N$	188°
Coridine	$(C_{10} H_{15})''' N$	211°
Rubidine	$(C_{11} H_{17})''' N$	230°
Viridine	$(C_{12} H_{19})''' N$	251°
Chinoline, or Leucoline	$C_9 H_7 N$	235°
Lepidine	$C_{10} H_9 N$	260°
Cryptidine	$C_{11} H_{11} N$	
Pyrrol	$C_4 H_5 N(?)$	
Hydrocyanic acid	$H C N$	

This is rather a formidable list of compounds; their names, too, are not always remarkable for smoothness and melodious character, although, I should not omit to state, they are tame and domestic when compared with some of the terms which chemists of late have been under the painful necessity of inventing and inflicting. You need not be afraid, however, that I shall trouble you with many details about these substances. Most of them, though highly interesting for more than one reason, more especially when considered from a purely scientific point of view, are of no importance for our present

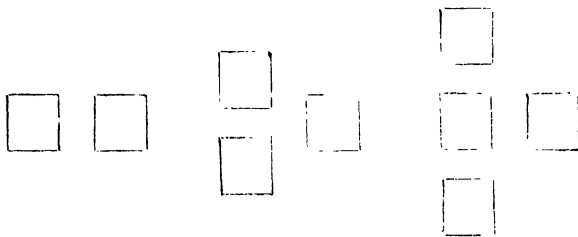
subject, and need not therefore specially be noticed. In fact, the only coal-derivatives which, in connection with Mauve and Magenta, claim our attention, are *Benzol*, *Phenol*, and *Aniline*; those certainly we must by-and-by examine somewhat more in detail.

But before doing so, you legitimately expect that I should endeavour to give you some idea of the nature of the process, in which this endless variety of compounds is generated from coal. Were I to tell you simply that coal consists of *Carbon*, *Hydrogen*, *Nitrogen*, *Oxygen*, and *Sulphur*, not to mention the ash which is left after combustion, and that you may therefore look upon coal as a sort of magazine of these several elements, capable, under the influence of heat, of associating in an infinity of forms and proportions, you would have learnt comparatively little. Let me attempt to convey to you a somewhat more precise idea of the processes involved in the distillation of coal. For this purpose you must allow me to remind you of some of the general results elaborated by the researches of chemists during the last ten years, which, at the first glance, appear but little connected with Mauve and Magenta.

The infinite number of substances, mineral, vegetal, or animal, which form our planet, variously as they are composed, may be referred,—chemists now pretty generally agree,—to a comparatively small number of *types of construction*. Opinions are divided respecting the actual number of these types, and even the choice of typical bodies is still a subject of discussion among chemists. But whatever the special views of particular schools may be, the number of types is always small, and among them almost invariably figure *Hydrogen*, *Water*, and *Ammonia*. The comprehension of the meaning attached by chemists to the term *types* may perhaps be facilitated to you by a glance at three models which I have had constructed for this purpose, and which for the sake of convenience I may be allowed to designate as *type-moulds*.*

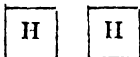
Chemists assume that the smallest particle of hydrogen, which

* These type-moulds consisted essentially of wire frames, presenting the outlines of cubes, associated, two, three, or four of them, in the manner indicated in the diagram

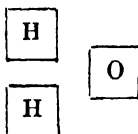


and capable of receiving zinc-cubes variously painted and marked, representing elementary and compound atoms.

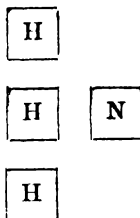
exists in the free state, or, to use the chemical phrase, the molecule of hydrogen, consists of two atoms of hydrogen. The first of our type-moulds then, charged as it is with *one* atom (one volume) of hydrogen, associated with *another* atom (one volume) of hydrogen, represents the *molecule of hydrogen*.



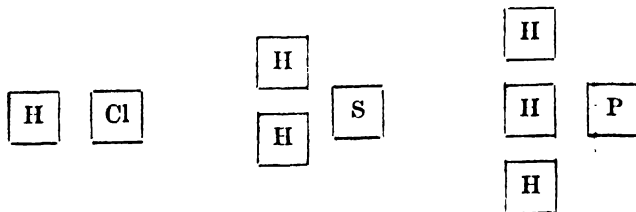
In water, as you know, we have *two* atoms (two volumes) of hydrogen, associated with *one* atom (one volume) of oxygen.* You are reminded of this fact by our second type-mould, which represents the *molecule of water*.



In ammonia, lastly, you have *three* atoms (three volumes) of hydrogen, united with *one* atom (one volume) of nitrogen, a form of construction which is recorded in our third type-mould representing the *molecule of ammonia*.



Nothing is easier now than to trace the derivation of other substances from hydrogen, from water, from ammonia. Let me remove from our three type-moulds one atom respectively of hydrogen, oxygen, and nitrogen, and fill the places thus vacated with atoms of chlorine, sulphur, and phosphorus, and I have, without giving you the slightest inconvenience, converted hydrogen into *hydrochloric acid*, water into *sulphuretted*, and ammonia into *phosphoretted hydrogen*.



* Equivalents used: H=1; O=16; S=32; C=12; N=14; Cl=33.5; &c.

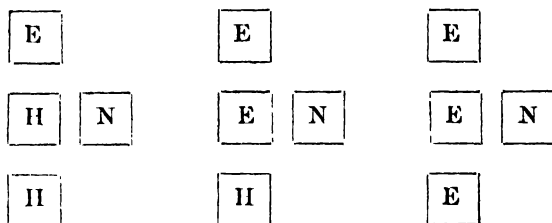
You observe the molecules of hydrochloric acid, of sulphuretted and of phosphoretted hydrogen respectively contain the same number of atoms which are present in the molecules of hydrogen, of water, and of ammonia. We have thus indicated that hydrochloric acid is constructed upon the hydrogen-type, sulphuretted hydrogen upon the water-type, phosphoretted hydrogen, lastly, upon the type of ammonia. The three bodies just considered were formed by the insertion of *elementary* atoms; but our type-moulds receive *compound* atoms with the same facility. Let me take as an illustration the compound atom *ethyl*, consisting of two atoms of carbon and five of hydrogen, ($C_2H_5 = E$), which is familiar to the members of the Royal Institution. By inserting *one* or *two* ethyl-atoms into the hydrogen-mould I generate the molecules of *ethylated hydrogen*, or *ethylated ethyl* (free *ethyl*).



In a similar manner, by introducing either *one* or *two* ethyl-atoms into water, I convert the molecule of water into the molecules of the two *ethylated waters*, *alcohol* and *ether*.



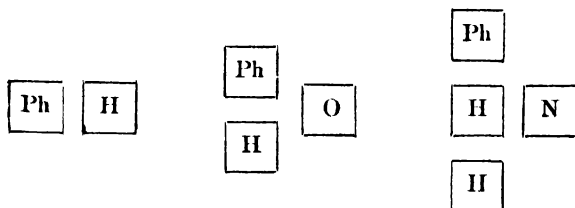
Displace, lastly, *one*, *two*, or *three* hydrogen-atoms in ammonia, by *one*, *two*, or *three* ethyl-atoms, and you give rise to the formation of the molecules of the three *ethylated ammonias*,



better known as *ethylamine*, *diethylamine*, and *triethylamine*.

At the risk of exhausting your patience, I repeat some of these changes with another compound atom of a composition differing from that of ethyl. These mauve-coloured cubes may represent a compound atom, containing six atoms of carbon and five of hydrogen ($C_6H_5 = Ph$), to which chemists have given the name of *phenyl*.

Charge each of our type-moulds with an atom of phenyl, and you accomplish the construction of *phenylated hydrogen*, *phenylated water*, and *phenylated ammonia*,



substances better known as *benzol*, *phenol*, and *aniline*;* and the existence of which in coal-tar-oil I have already pointed out to you.

But it is time for us to return to the point from which we started. What has the recognition of our types to do with the distillation of coal? In what manner do they explain the formation of the variety of substances generated in this process. In coal we have the elements of the three types of matter, and we find that hydrogen, water, and ammonia are in fact produced to a very appreciable extent during its distillation. The quantity of free hydrogen is generally small; moreover, mixed as it is with the carbonetted hydrogens of coal gas, its presence among the products of distillation of coal is not easily demonstrated by experiment. Water and ammonia, on the other hand, are abundantly generated, and nothing is easier than to exhibit their production. In fact, the coal-tar-oil which we have produced in our distillation-experiment, is covered, as you observe, with a layer of water, and the application of test-papers to the latter shows that it contains a large amount of ammonia. Now consider that our types are generated from coal in the presence of large quantities of carbon and hydrogen, two elements which, in proportions varying to an almost unlimited extent, may aggregate under the influence of heat to compound atoms similar to ethyl and phenyl; remember, moreover, that these atoms are capable of displacing, partly or entirely, the hydrogen of our types, and you will realize without difficulty the number of compounds which may be formed by the distillation of coal; I say which may be formed, for the diagram which I have exhibited to you enumerates only the bodies which have actually been obtained; but every day brings forth new substances. It is obvious that the nature of the compound atoms generated must, in a measure, depend upon the composition of the coal distilled. The composition of coal, however, varies between very considerable limits. In the subjoined diagram I give you a synopsis of the results obtained in the analysis of several specimens of coal.

* Large specimens of these three substances were exhibited upon the table.

ANALYSIS OF DIFFERENT COALS.

Locality of Coal.	100 PARTS OF DRY COAL						
	Contain					Leave	
	Carbon.	Hydrogen.	Nitrogen.	Sulphur.	Oxygen.	Ash.	Coke.
Anthracite, Wales . . .	91.44	3.36	0.21	0.79	2.58	1.52	92.20
" " " " " " " " " " " "	90.39	3.28	0.83	0.91	2.97	1.61	92.10
Caking Coal, Newcastle	81.41	5.83	2.05	0.75	7.90	2.07	66.70
Cannel Coal, Wigan . . .	80.07	5.53	2.12	1.50	8.09	2.70	60.36
Coal, Wolverhampton . .	78.57	5.29	1.84	0.39	12.88	10.30	57.21
Wallsend, Elgin	76.09	5.22	1.41	1.53	5.05	10.70	58.40
St. Helen's, Lancashire	75.80	5.21	1.92	0.90	11.89	5.17	65.50
Methill Brown Coal . . .	65.96	7.78	0.96	0.75	9.23	15.32	
Bohemian Brown Coal . .	55.59	4.16	19.06			21.19	

A glance at this diagram shows you that the carbon in the several specimens varies by more than 30 per cent., being 91.4 in Welsh anthracite and 55.5 in Bohemian brown coal. Similar, though less marked, discrepancies are perceptible in the other constituents. If you recollect, in addition, that the nature of the compound atoms generated in the distillation of coal must be influenced, moreover, by the temperature, which again oscillates between limits widely apart, you cannot fail to perceive that the destructive distillation of coal must be an almost inexhaustible source of new compounds.

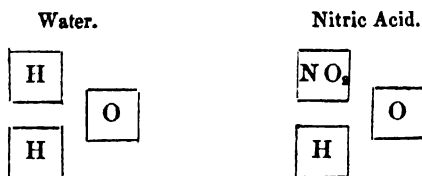
The separation of the individual substances from the complex mixture called coal-tar-oil appears, at the first glance, to present almost insurmountable obstacles. But the principles made use of for this purpose are very simple. The individual compounds contained in coal-tar-oil may be separated in a great measure by distillation, their boiling points varying, as may be seen by a glance at the diagram, to a considerable extent. But additional means of purification offer themselves in the different deportment which these substances exhibit under the influence of chemical agents. I could not perhaps, in this respect, bring under your notice a more instructive illustration than the behaviour with acids and bases of the three coal-tar-oil-constituents, repeatedly quoted. Benzol, phenol, and aniline may thus easily be separated. To demonstrate this point experimentally, two glass cylinders have been half-filled with benzol, two others with phenol, and two further ones with aniline; a solution of litmus having, moreover, been added, each of the three substances is treated in one cylinder with acid, in the other with alkali. In the case of benzol, you observe, the indifferent hydrocarbon, insoluble both in acid and alkali, floating colourless upon the coloured liquid; phenol, being an acid water-derivative, is not acted upon by the acid, but readily dissolves in the alkali; aniline, lastly, being a well-defined ammonia-

derivative, exhibits the converse deportment, resisting the action of the alkali and forming a homogeneous solution with the acid.

Each of the three coal-tar-oil-constituents which I have mentioned, and of which you have characteristic specimens upon the lecture-table, has received important applications in the arts and manufactures. Benzol is the most convenient solvent for caoutchouc; as an agent for removing oil and grease it has become an ordinary household article; phenol, when treated with nitric acid, yields us a beautiful yellow dye, called by chemists carbazotic acid; but the practical interest attached to phenol you will more immediately appreciate if I tell you that this compound presents the greatest analogy with creosote, a substance, I am afraid, but too well known to most of us,—a considerable portion of the creosote of commerce being in fact simply phenol; aniline, lastly, is the source of Mauve and Magenta, and must therefore claim our attention more particularly this evening.

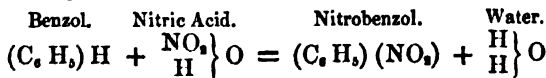
The amount of aniline which exists in coal-tar is very limited; a preparation from this source upon a sufficiently large scale could never be attempted. Fortunately, chemists are in the possession of a series of processes by which aniline may be produced in any quantity. Benzol, the phenylated hydrogen, may readily be converted into aniline, the phenylated ammonia. Let us examine this transformation experimentally.

Benzol is readily attacked by fuming nitric acid; it dissolves in it, producing a liquid of a deep-red colour. On addition of water this liquid deposits a heavy yellow oil, collecting at the bottom of the cylinder, perfectly different from benzol, which floats on the surface of the water. The reaction will be intelligible to you, if I remind you that nitric acid when referred to our types, must be viewed as a water-derivative; it is water in which, for one of the elementary hydrogen-atoms, there has been substituted a compound atom, consisting of nitrogen and oxygen.

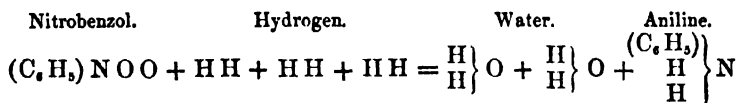


Modern chemistry, you observe, returns to the conceptions of former ages, which in the name *aqua fortis* appear to have anticipated in a measure our present notions.

When nitric acid acts on benzol, an interchange takes place between the elementary atom of the latter and the compound atom of the former, *nitrobenzol*, the heavy yellow liquid, which we have produced, and water being formed:—



The transformation of benzol into nitrobenzol, discovered by Mitscherlich, is only a preparatory operation for the production of aniline. The method of converting nitrobenzol into aniline was discovered by Zinin. It consists in submitting nitrobenzol to the action of nascent hydrogen. Under the influence of this agent, the compound atom N O_2 , which in nitrobenzol is associated with phenyl, is decomposed; its oxygen is converted into water, the residue of nitrogen and phenyl assimilating the necessary quantity of hydrogen to form phenylated ammonia, or aniline.



The hydrogen necessary for this transformation may be furnished by numerous processes. The most convenient method for our purpose consists in submitting nitrobenzol to the action of metallic iron and acetic acid, a process first proposed by M. Béchamp. I mix the three bodies in a glass retort, and on application of a gentle heat you observe how immediately a most powerful reaction manifests itself. Let us hasten to connect the retort with a condenser; I have removed the gas-burner from the retort; nevertheless the reaction continues, and a considerable quantity of water, covering an oily layer, has already accumulated in the receiver. This oily liquid is *aniline*. We recognize it at once by its peculiar deportment with a solution of chloride of lime. On pouring a single drop of our distillate into this beaker which contains a solution of chloride of lime, a splendid purple cloud is almost instantaneously diffused throughout the liquid. You perceive we are approaching our subject. The beautiful colour which aniline strikes with a solution of chloride of lime has been long known. A solution of bleaching powder has always been used as a test for aniline; indeed it was by this colour-reaction that the presence of our compound in coal-tar-oil was first pointed out, a fact recorded in the name *Kyanol* (blue oil), originally given to aniline prepared from coal-tar. Several other oxidizing agents, chromic acid for instance, were likewise known to produce coloured compounds from aniline; but all the colours thus obtained were of an highly ephemeral character. Observe how the purple cloud which I produced by means of chloride of lime has rapidly changed to a dingy reddish precipitate. It was Mr. W. Perkin who had first the happy idea of investigating the circumstances under which this beautiful purple might be prepared in a form permanent and applicable for the purposes of the dyer. He succeeded in isolating this colour by submitting, under appropriately selected circumstances, aniline to the action of bichromate of potassium and sulphuric acid.

Here then you have, step by step, the development of this new and important branch of chemical industry.

Through the kindness of my friend, Mr. Perkin, I am enabled to exhibit to you magnificent specimens of his aniline purple, or Mauve,

in the dry state and in solution. This brown lump, with the remarkable coppery lustre, is Mauve in the solid state; its extraordinary tinctorial powers will be appreciated, if I tell you that this beautiful violet-coloured solution contains not more than $\frac{1}{10}$ of a grain of Mauve in one gallon of alcohol; you will also understand the considerable commercial value of this substance. Weight for weight, I am told by Mr. Perkin, this colouring matter, when pure, is sold at the price of metallic platinum.

Very little is known regarding the chemical nature of Mauve; its composition is not yet made out, and as a matter of course the process by which it is formed from aniline remains as yet perfectly unexplained.

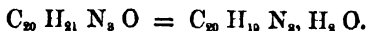
Magenta is one of the fancy names given to the splendid crimson, which is likewise generated from aniline by the action of oxidizing agents. This substance was first observed in purely scientific researches, and more especially in the action of tetrachloride of carbon upon aniline. To a French chemist, M. Verguin, the merit is due of having for the first time obtained this substance on a larger scale; he produced it by the action of tetrachloride of tin on aniline. Numerous other processes were subsequently suggested, among which treatment of aniline with chloride or nitrate of mercury, with arsenic acid, and many other substances may be mentioned. Magenta, often called fuchsine, roseine, &c., soon became an article of large consumption. A great impetus to this new branch of industry was given in France by Messrs. Renard and Franc, who were the first to manufacture the new article on a commercial scale; in this country, very soon afterwards, Messrs. Simpson, Maule, and Nicholson engaged with great spirit in the manufacture of this splendid dye, the production of which has attained already colossal proportions. To Mr. E. C. Nicholson more especially belongs the credit of having developed this new industry to an unprecedented degree of perfection.

Before proceeding, however, let me show you the formation of Magenta by experiment. Among the many processes which I might adopt for this purpose I select the action of corrosive sublimate upon aniline, not because I consider this process superior to the others,—it is, in fact, inferior to many,—but because it is, perhaps, the best adapted for a lecture-experiment. This white powder is chloride of mercury (corrosive sublimate); a small portion of this salt I mix in a test-tube with perfectly colourless aniline. Let us stir the mixture with a glass rod until it is converted into a perfectly homogeneous liquid paste. This paste is still colourless; but on gently heating it by a gas-burner, it instantaneously assumes a splendid crimson of the greatest intensity, a single drop of the liquid being capable of deeply colouring a large beaker filled with alcohol.

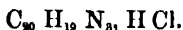
In all the processes which convert aniline into colouring matters, a considerable number of secondary products are generated, which it is rather difficult to separate from the principal product of the reaction. These difficulties have been most perfectly overcome by Mr. Nicholson,

who has succeeded in obtaining Magenta in a state of absolute purity. Chemists have thus been enabled to analyze this substance, and to lift, at all events, the corner of the veil which still covers the mysterious formation of the coloured derivatives of aniline.

In the pure state Magenta is a fine crystalline, and, remarkably enough, perfectly colourless, or only slightly tinted body, which is represented by the formula—



Rosaniline (this is the name by which chemists designate the colourless body) is a base, or ammonia-derivative, which forms a series of splendid salts. With hydrochloric acid, for instance, it produces a beautifully crystalline salt of the formula—



It is in the state of saline combination that rosaniline acts as a crimson dye. Into this shallow porcelain dish I have thrown a few crystals of rosaniline, which at a distance you scarcely perceive; I now pour upon these crystals a small quantity of acetic acid, when on gently heating the dish the crimson colour instantaneously appears. But it is only *in solution* that even the salts of rosaniline are crimson-coloured; on slowly *evaporating* their solution, the red colour entirely *vanishes*, and a splendid green crystalline substance remains, presenting in an extraordinary manner the beautiful metal-lustre which distinguishes the wings of the rose-beetle. Together with all the products involved in the manufacture of aniline and aniline-dyes, my friend Mr. Nicholson has placed before you the finest series of rosaniline-salts which has ever been produced; and not content with this display, he was kind enough to send us a specimen of acetate of rosaniline, such as no mortal eye has ever seen before. The specimen may be literally called the *Crown of Magenta*.* Crowns are always expensive articles, and often are the cost and trouble of getting them greater than their actual value. This remark applies in a measure to the Crown of Magenta. For the benefit of those who are fond of big figures—and who is not a little afflicted with this weakness?—I may state that the crown was grown in a vessel containing not less than 8000*l.* worth of Magenta, the crown itself being worth upwards of 100*l.*

Having now explained the several stages of transition through which coal has to pass before it becomes either Mauve or Magenta, it may be of some interest to you to know the proportion which the finished dye bears to the coal from which it is derived. A set of specimens for which I am likewise indebted to Mr. Nicholson, is most instructive in this respect. Observe, it commences with a large mass of coal, weighing not less than 100 lbs.; the bottles which follow contain the coal-tar-oil, naphtha, benzol, nitrobenzol, and aniline,

* An immense aggregate of sparkling green octohedral crystals,—some of them as much as an inch in diameter,—deposited upon a large wire frame having the shape of an elegant crown, was here exhibited.

obtainable in succession from 100 lbs. of coal; remark how they gradually diminish in size, and how small, I might almost say insignificant, appears the bulk of Magenta finally obtained. But compare the bulk of wool which this minute quantity will dye. It approximates to the bulk of coal with which we started. This comparison evinces perhaps sufficiently the extraordinary tinctorial power which this class of dyes possesses; but a very simple experiment may possibly convey to you this idea even in a more impressive manner. The white paper which covers this large frame has been dusted over with a minute quantity of Mauve; a second one is treated in a similar manner with Magenta. The quantity of colouring matter is so small, that the paper has retained its original white colour, but observe how it changes, when I dash a beaker full of spirit against these squares; immediately the lovely purple of Mauve is developed upon one of them, whilst the other one exhibits the dazzling crimson of Magenta.

But let us now proceed to illustrate the mode of dyeing. For this purpose I introduce silk and wool, both unspun and woven, in succession, into solutions of Mauve, of Magenta, and lastly, of a splendid new Purple, lately discovered by Mr. Nicholson. Observe the extraordinary facility with which the coal-tar-colours are fixed both on wool and silk. These materials require no previous preparation, being dyed, in fact, simply by dipping, without the aid of any mordant. Silk and wool are animal substances; vegetal materials, such as cotton and linen, unless previously submitted to a special treatment, are scarcely affected by these dyes. This fact admits of being beautifully illustrated by dyeing linen fabrics on which ornaments have been embroidered in silk ribbon. The articles, when coming out of the bath, appear uniformly dyed; but by washing first in pure water and then in dilute ammonia, the colour rapidly vanishes from the linen ground, leaving the silk embroidery in brilliant colours. This extraordinary predilection of the aniline-colours for animal substances is, moreover, strikingly illustrated by the condition of my hands, which by this time have acquired a thoroughly Magentic appearance. Fortunately, the coal-tar-colours are unable to resist the action of chloride of lime, and I have therefore only to immerse my hands for a moment into a solution of bleaching powder.

Already the colour on my hand, you observe, has gone, but with the colour, I am afraid, my time too. Let me endeavour to bring this lecture to a conclusion. I have fulfilled in a measure the promise which I gave you at the commencement of this discourse. We have crossed together the extensive field which stretches between *coal* and *colour*. I am impressed, deeply impressed with the clumsiness of my performance as your guide, but I hope that the interest attached to the territory we have explored may, to some extent at all events, have indemnified you for the imperfection of my explanations, and that you leave the Royal Institution this evening with the kind of feeling every one of us has more than once experienced after travelling in similar company over a beautiful country—the guide is forgotten, but the impression of the scenery remains.

Having gone thus far, you may think that it is fully time for me to make my bow. But I venture, even at this late hour, to dwell for a moment on the *moral* of the story which I have told you, though you may feel disposed to consider this story rather a highly coloured one.

The material which I had to condense, I might almost say to force, into the short space of an hour, has been overwhelming; and whilst explaining the formation of the various substances which I had to describe, whilst illustrating their properties by experiment, I have scarcely had time to glance at the history of our subject. This history is not without interest. You readily perceive that a branch of industry like the one I have endeavoured to sketch could not possibly have risen like Minerva from the head of Jupiter—a sudden inspiration happily realized. The time, the toil, the thought of a host of inquirers were necessary to accomplish so remarkable an achievement. You cannot expect me at this late hour to examine minutely into this part of the subject, but I must not take leave of you without alluding to some facts which cannot fail to rivet the lively interest of the Members of this Institution. Let me tell you then that Mauve and Magenta are essentially Royal Institution colours; the foundation of this new industry was laid in Albemarle Street. Benzol, which I have so repeatedly mentioned,—benzol, which may be looked upon as the raw material, capable, under the influence of chemical agents, of assuming such wonderful shapes,—benzol is the discovery of our great master, may I not add of our kind friend, Mr. Faraday. This volume, 'The Philosophical Transactions for 1825,' contains the description of his experiments. In 1825, thirty-seven years ago, the laboratory of the Royal Institution witnessed the birth of this remarkable body. Yesterday, under the auspices of Mr. Anderson, I invaded the same laboratory, a diligent search was made, and in my hand I hold the trophies of our expedition, the original specimens of benzol which Mr. Faraday prepared. In thus reminding you of one of the early labours of Mr. Faraday,—which, owing to the number and vastness of his subsequent discoveries, appears almost to have escaped from his memory like a tradition of years gone by,—I have opened a glorious page in the glorious history of the Royal Institution. Benzol has furnished us Mauve and Magenta, but it has done much more than this. Ever since chemistry became endowed with this wonderful body, benzol has been the carrier of many of the leading ideas in our science. In the hands of Mitscherlich, Zinin, Gerhardt and Laurent, in the hands of Charles Mansfield—never to be forgotten by his friends—and many others, benzol has been a powerful lever for the advancement of chemical science. Benzol and its derivatives form one of the most interesting chapters in organic chemistry, the progress of which is intimately allied with the history of this compound.

But what has the history of benzol to do with the moral of Mauve and Magenta? Well, ladies and gentlemen, ask Mr. Faraday; ask him what in 1825 was his object in examining benzol. I have perhaps no right to answer this question in Mr. Faraday's presence; but I venture to say that we owe his remarkable inquiry to the pure delight he felt

in the elaboration of truth. It was in the same spirit that his successors continued the work. Patiently they elicited fact after fact; observation was recorded after observation; it was the labour of love performed for the sake of truth; ultimately, by the united efforts of so many ardent inquirers, exerted year after year in the same direction, the chemical history of benzol and its derivatives had been traced. The scientific foundation having thus been laid, the time of application had arrived, and by one bound, as it were, these substances, hitherto exclusively the property of the philosopher, appear in the market-place of life.

Need I say any more? The moral of Mauve and Magenta is transparent enough. I read it in your eyes,—we understand each other. Whenever in future one of your chemical friends, full of enthusiasm, exhibits and explains to you his newly-discovered compound, you will not cool his noble ardour by asking him that most terrible of all questions, “What is its use? Will your compound bleach or dye? Will it shave? May it be used as a substitute for leather?” Let him quietly go on with his work. The dye, the lather, the leather will make their appearance in due time. Let him, I repeat it, perform *his* task. Let him indulge in the pursuit of truth,—of truth pure and simple,—of truth not for the sake of Mauve, not for the sake of Magenta—let him pursue truth for the sake of truth!

[A. W. II.]

Friday, June 6, 1862.

THE DUKE OF NORTHUMBERLAND, K.G. F.R.S. President,
in the Chair.

JOHN TYNDALL, Esq. F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION.

On Force.

THE existence of the International Exhibition suggested to our Honorary Secretary the idea of devoting the Friday evenings after Easter of the present year to discourses on the various agencies on which the material strength of England is based. He wished to make iron, coal, cotton, and kindred matters, the subjects of these discourses; opening the series by a discourse on the Great Exhibition itself; and he wished me to finish the series by a discourse on "Force" in general. For some months I thought over the subject at intervals, and had devised a plan of dealing with it; but three weeks ago I was induced to swerve from this plan, for reasons which shall be made known towards the conclusion of the discourse.

We all have ideas more or less distinct regarding force; we know in a general way what muscular force means, and each of us would less willingly accept a blow from a pugilist than have his ears boxed by a lady. But these general ideas are not now sufficient for us; we must learn how to express numerically the exact mechanical value of the two blows; this is the first point to be cleared up.

A sphere of lead weighing 1 lb. was suspended at a height of 16 feet above the theatre floor. It was liberated, and fell by gravity. That weight required exactly a second to fall to the earth from that elevation; and the instant before it touched the earth, it had a velocity of 32 feet a second. That is to say, if at that instant the earth were annihilated, and its attraction annulled, the weight would proceed through space at the uniform velocity of 32 feet a second.

Suppose that instead of being pulled downward by gravity, the weight is cast upward in opposition to the force of gravity, with what velocity must it start from the earth's surface in order to reach a height of 16 feet? With a velocity of 32 feet a second. This velocity imparted to the weight by the human arm, or by any other mechanical means, would carry the weight up to the precise height from which it has fallen.

Now the lifting of the weight may be regarded as so much mechanical work. I might place a ladder against the wall, and carry the

weight up a height of 16 feet; or I might draw it up to this height by means of a string and pulley, or I might suddenly jerk it up to a height of 16 feet. The amount of work done in all these cases, as far as the raising of the weight is concerned, would be absolutely the same. The absolute amount of work done depends solely upon two things: first of all, on the quantity of matter that is lifted; and secondly, on the height to which it is lifted. If you call the quantity or mass of matter m , and the height through which it is lifted h , then the product of m into h , or mh , expresses the amount of work done.

Supposing, now, that instead of imparting a velocity of 32 feet a second to the weight we impart twice this speed, or 64 feet a second. To what height will the weight rise? You might be disposed to answer, "To twice the height;" but this would be quite incorrect. Both theory and experiment inform us that the weight would rise to four times the height: instead of twice 16, or 32 feet, it would reach four times 16, or 64 feet. So also, if we treble the starting velocity, the weight would reach nine times the height; if we quadruple the speed at starting, we attain sixteen times the height. Thus, with a velocity of 128 feet a second at starting, the weight would attain an elevation of 256 feet. Supposing we augment the velocity of starting seven times, we should raise the weight to 49 times the height, or to an elevation of 784 feet.

Now the work done—or, as it is sometimes called, the *mechanical effect*—as before explained, is proportional to the height, and as a double velocity gives four times the height, a treble velocity nine times the height, and so on, it is perfectly plain that the mechanical effect increases as the square of the velocity. If the mass of the body be represented by the letter m , and its velocity by v , then the mechanical effect would be represented by mv^2 . In the case considered, I have supposed the weight to be cast upward, being opposed in its upward flight by the resistance of gravity; but the same holds true if I send the projectile into water, mud, earth, timber, or other resisting material. If, for example, you double the velocity of a cannon-ball, you quadruple its mechanical effect. Hence the importance of augmenting the velocity of a projectile, and hence the philosophy of Sir William Armstrong in using a 50 lb. charge of powder in his recent striking experiments.

The measure then of mechanical effect is the mass of the body multiplied by the square of its velocity.

Now in firing a ball against a target the projectile, after collision, is often found hissing hot. Mr. Fairbairn informs me that in the experiments at Shoeburyness it is a common thing to see a flash of light, even in broad day, when the ball strikes the target. And if I examine my lead weight after it has fallen from a height I also find it heated. Now here experiment and reasoning lead us to the remarkable law that the amount of heat generated, like the mechanical effect, is proportional to the product of the mass into the square of the velocity. Double your mass, other things being equal, and you double your

amount of heat; double your velocity, other things remaining equal, and you quadruple your amount of heat. Here then we have common mechanical motion destroyed and heat produced. I take this violin bow and draw it across this string. You hear the sound. That sound is due to motion imparted to the air, and to produce that motion a certain portion of the muscular force of my arm must be expended. We may here correctly say, that the mechanical force of my arm is converted into music. And in a similar way we say that the impeded motion of our descending weight, or of the arrested cannon-ball, is converted into heat. The mode of motion changes, but it still continues motion; *the motion of the mass is converted into a motion of the atoms of the mass*; and these small motions, communicated to the nerves, produce the sensation which we call heat. We, moreover, know the amount of heat which a given amount of mechanical force can develope. Our lead ball, for example, in falling to the earth generated a quantity of heat sufficient to raise the temperature of its own mass three-fifths of a Fahrenheit degree. It reached the earth with a velocity of 32 feet a second, and forty times this velocity would be a small one for a rifle bullet; multiplying $\frac{4}{5}$ ths by the square of 40, we find that the amount of heat developed by collision with the target would, if wholly concentrated in the lead, raise its temperature 960 degrees. This would be more than sufficient to fuse the lead. In reality, however, the heat developed is divided between the lead and the body against which it strikes; nevertheless, it would be worth while to pay attention to this point and to ascertain whether rifle bullets do not, under some circumstances, show signs of fusion.

From the motion of sensible masses, by gravity and other means, the speaker passed to the motion of atoms towards each other by chemical affinity. A collodion balloon filled with a mixture of chlorine and hydrogen was hung in the focus of a parabolic mirror, and in the focus of a second mirror 20 ft. distant a strong electric light was suddenly generated; the instant the light fell upon the balloon, the atoms within it fell together with explosion, and hydro-chloric acid was the result. The burning of charcoal in oxygen was an old experiment, but it had now a significance beyond what it used to have; we now regard the act of combination on the part of the atoms of oxygen and coal exactly as we regard the clashing of a falling weight against the earth. And the heat produced in both cases is referable to a common cause. This glowing diamond, which burns in oxygen as a star of white light, glows and burns in consequence of the falling of the atoms of oxygen against it. And could we measure the velocity of the atoms when they clash, and could we find their number and weight, multiplying the mass of each atom by the square of its velocity, and adding all together, we should get a number representing the exact amount of heat developed by the union of the oxygen and carbon.

Thus far we have regarded the heat developed by the clashing of sensible masses and of atoms. Work is expended in giving motion to these atoms or masses, and heat is developed. But we reverse this

process daily, and by the expenditure of heat execute work. We can raise a weight by heat; and in this agent we possess an enormous store of mechanical power. This pound of coal, which I hold in my hand, produces by its combination with oxygen an amount of heat which, if mechanically applied, would suffice to raise a weight of 100 lbs. to a height of 20 miles above the earth's surface. Conversely, 100 lbs. falling from a height of 20 miles, and striking against the earth, would generate an amount of heat equal to that developed by the combustion of a pound of coal. Wherever work is done by heat, heat disappears. A gun which fires a ball is less heated than one which fires blank cartridge. The quantity of heat communicated to the boiler of a working steam-engine is greater than that which could be obtained from the re-condensation of the steam after it had done its work; and the amount of work performed is the exact equivalent of the amount of heat lost. Mr. Smyth informed us in his interesting discourse, that we dig annually 84 millions of tons of coal from our pits. The amount of mechanical force represented by this quantity of coal seems perfectly fabulous. The combustion of a single pound of coal, supposing it to take place in a minute, would be equivalent to the work of 300 horses; and if we suppose 108 millions of horses working day and night with unimpaired strength, for a year, their united energies would enable them to perform an amount of work just equivalent to that which the annual produce of our coal-fields would be able to accomplish.

Comparing the energy of the force with which oxygen and carbon unite together, with ordinary gravity the chemical affinity seems almost infinite. But let us give gravity fair play; let us permit it to act throughout its entire range. Place a body at such a distance from the earth that the attraction of the earth is barely sensible, and let it fall to the earth from this distance. It would reach the earth with a final velocity of 36,747 feet in a second; and on collision with the earth the body would generate about twice the amount of heat generated by the combustion of an equal weight of coal. We have stated that by falling through a space of 16 feet our lead bullet would be heated three-fifths of a degree; but a body falling from an infinite distance has already used up 1,299,999 parts out of 1,300,000 of the earth's pulling power, when it has arrived within 16 feet of the surface; on this space only $\frac{1}{1300000}$ ths of the whole force is exerted.

Let us now turn our thoughts for a moment from the earth towards the sun. The researches of Sir John Herschel and M. Pouillet have informed us of the annual expenditure of the sun as regards heat; and by an easy calculation we ascertain the precise amount of the expenditure which falls to the share of our planet. Out of 2300 million parts of light and heat the earth receives one. The whole heat emitted by the sun in a minute would be competent to boil 12,000 millions of cubic miles of ice-cold water. How is this enormous loss made good? Whence is the sun's heat derived, and by what means is it maintained? No combustion, no chemical affinity with which we are acquainted

would be competent to produce the temperature of the sun's surface. Besides, were the sun a burning body merely, its light and heat would assuredly speedily come to an end. Supposing it to be a solid globe of coal, its combustion would only cover 4600 years of expenditure. In this short time it would burn itself out. What agency then can produce the temperature and maintain the outlay? We have already regarded the case of a body falling from a great distance towards the earth, and found that the heat generated by its collision would be twice that produced by the combustion of an equal weight of coal. How much greater must be the heat developed by a body falling towards the sun! The maximum velocity with which a body can strike the earth is about 7 miles in a second; the maximum velocity with which it can strike the sun is 390 miles in a second. And as the heat developed by the collision is proportional to the square of the velocity destroyed, an asteroid falling into the sun with the above velocity would generate about 10,000 times the quantity of heat generated by the combustion of an asteroid of coal of the same weight. Have we any reason to believe that such bodies exist in space, and that they may be raining down upon the sun? The meteorites flashing through the air are small planetary bodies, drawn by the earth's attraction, and entering our atmosphere with planetary velocity. By friction against the air they are raised to incandescence and caused to emit light and heat. At certain seasons of the year they shower down upon us in great numbers. In Boston 240,000 of them were observed in nine hours. There is no reason to suppose that the planetary system is limited to "vast masses of enormous weight;" there is every reason to believe that space is stocked with smaller masses, which obey the same laws as the large ones. That lenticular envelope which surrounds the sun, and which is known to astronomers as the Zodiacal light, is probably a crowd of meteors; and moving as they do in a resisting medium they must continually approach the sun. Falling into it, they would be competent to produce the heat observed, and this would constitute a source from which the annual loss of heat would be made good. The sun, according to this hypothesis, would be continually growing larger; but how much larger? Were our moon to fall into the sun it would develop an amount of heat sufficient to cover one or two years' loss; and were our earth to fall into the sun a century's loss would be made good. Still, our moon and our earth, if distributed over the surface of the sun, would utterly vanish from perception. Indeed, the quantity of matter competent to produce the necessary effect would, during the range of history, produce no appreciable augmentation in the sun's magnitude. The augmentation of the sun's attractive force would be more appreciable. However this hypothesis may fare as a representant of what is going on in nature, it certainly shows how a sun might be formed and maintained by the application of known thermo-dynamic principles.

Our earth moves in its orbit with a velocity of 68,040 miles an hour. Were this motion stopped, an amount of heat would be de-

veloped sufficient to raise the temperature of a globe of lead of the same size as the earth 384,000 degrees of the centigrade thermometer. It has been prophesied that "the elements shall melt with fervent heat." The earth's own motion embraces the conditions of fulfilment; stop that motion, and the greater part, if not the whole, of her mass would be reduced to vapour. If the earth fell into the sun, the amount of heat developed by the shock would be equal to that developed by the combustion of 6435 earths of solid coal.

There is one other consideration connected with the permanence of our present terrestrial conditions, which is well worthy of our attention. Standing upon one of the London bridges, we observe the current of the Thames reversed, and the water poured upward twice a-day. The water thus moved rubs against the river's bed and sides, and heat is the consequence of this friction. The heat thus generated is in part radiated into space, and then lost, as far as the earth is concerned. What is it that supplies this incessant loss? The earth's rotation. Let us look a little more closely at the matter. Imagine the moon fixed, and the earth turning like a wheel from west to east in its diurnal rotation. Suppose a high mountain on the earth's surface; on approaching the moon's meridian, that mountain is, as it were, laid hold of by the moon, and forms a kind of handle by which the earth is pulled more quickly round. But when the meridian is passed the pull of the moon on the mountain would be in the opposite direction, it now tends to diminish the velocity of rotation as much as it previously augmented it; and thus the action of all fixed bodies on the earth's surface is neutralized. But suppose the mountain to lie *always* to the east of the moon's meridian, the pull then would be always exerted against the earth's rotation, the velocity of which would be diminished in a degree corresponding to the strength of the pull. *The tidal wave occupies this position*—it lies always to the east of the moon's meridian, and thus the waters of the ocean are in part dragged as a brake along the surface of the earth; and as a brake they must diminish the velocity of the earth's rotation. The diminution, though inevitable, is, however, too small to make itself felt within the period over which observations on the subject extend. Supposing then that we turn a mill by the action of the tide, and produce heat by the friction of the mill-stones; that heat has an origin totally different from the heat produced by another mill which is turned by a mountain stream. The former is produced at the expense of the earth's rotation, the latter at the expense of the sun's radiation.

The sun, by the act of vaporization, lifts mechanically all the moisture of our air. It condenses and falls in the form of rain,—it freezes and falls as snow. In this solid form it is piled upon the Alpine heights, and furnishes materials for the glaciers of the Alps. But the sun again interposes, liberates the solidified liquid, and permits it to roll by gravity to the sea. The mechanical force of every river in the world as it rolls towards the ocean, is drawn from the heat of the sun. No streamlet glides to a lower level without having been

first lifted to the elevation from which it springs by the mighty power of the sun. The energy of winds is also due entirely to the sun; but there is still another work which he performs, and his connection with which is not so obvious. Trees and vegetables grow upon the earth, and when burned they give rise to heat, and hence to mechanical energy. Whence is this power derived? You see this oxide of iron, produced by the falling together of the atoms of iron and oxygen; here also is a transparent gas which you cannot now see—carbonic acid gas—which is formed by the falling together of carbon and oxygen. These atoms thus in close union resemble our lead weight while resting on the earth; but I can wind up the weight and prepare it for another fall, and so these atoms can be wound up, separated from each other, and thus enabled to repeat the process of combination. In the building of plants carbonic acid is the material from which the carbon of the plant is derived; and the solar beam is the agent which tears the atoms asunder, setting the oxygen free, and allowing the carbon to aggregate in woody fibre. Let the solar rays fall upon a surface of sand; the sand is heated, and finally radiates away as much heat as it receives; let the same beams fall upon a forest, the quantity of heat given back is less than the forest receives, for the energy of a portion of the sunbeams is invested in building up the trees in the manner indicated. Without the sun the reduction of the carbonic acid cannot be effected, and an amount of sunlight is consumed exactly equivalent to the molecular work done. Thus trees are formed; thus the cotton on which Mr. Bazley discoursed last Friday is formed. I ignite this cotton, and it flames; the oxygen again unites with its beloved carbon; but an amount of heat equal to that which you see produced by its combustion was sacrificed by the sun to form that bit of cotton.

But we cannot stop at vegetable life, for this is the source, mediate or immediate, of all animal life. The sun severs the carbon from its oxygen; the animal consumes the vegetable thus formed, and in its arteries a reunion of the severed elements take place, and produce animal heat. Thus, strictly speaking, the process of building a vegetable is one of winding up; the process of building an animal is one of running down. The warmth of our bodies, and every mechanical energy which we exert, trace their lineage directly to the sun. The fight of a pair of pugilists, the motion of an army, or the lifting of his own body up mountain slopes by an Alpine climber, are all cases of mechanical energy drawn from the sun. Not, therefore, in a poetical, but in a purely mechanical sense, are we children of the sun. Without food we should soon oxidize our own bodies. A man weighing 150 lbs. has 64 lbs. of muscle; but these, when dried, reduce themselves to 15 lbs. Doing an ordinary day's work, for 80 days, this mass of muscle would be wholly oxidized. Special organs which do more work would be more quickly oxidized: the heart, for example, if entirely unsustained, would be oxidized in about a week. Take the amount of heat due to the direct oxidation of a given amount of food; a less

amount of heat is developed by this food in the working animal frame, and the missing quantity is the exact equivalent of the mechanical work which the body accomplishes.

I might extend these considerations; the work, indeed, is done to my hand—but I am warned that I have kept you already too long. To whom then are we indebted for the striking generalizations of this evening's discourse? All that I have laid before you is the work of a man of whom you have scarcely ever heard. All that I have brought before you has been taken from the labours of a German physician, named Mayer. Without external stimulus, and pursuing his profession as town physician in Heilbronn, this man was the first to raise the conception of the interaction of natural forces to clearness in his own mind. And yet he is scarcely ever heard of in scientific lectures, and even to scientific men his merits are but partially known. Led by his own beautiful researches, and quite independent of Mayer, Mr. Joule published his first Paper on the 'Mechanical Value of Heat,' in 1843; but in 1842 Mayer had actually calculated the mechanical equivalent of heat from data which a man of rare originality alone could turn to account. From the velocity of sound in air Mayer determined the mechanical equivalent of heat. In 1845 he published his Memoir on 'Organic Motion,' and applied the mechanical theory of heat in the most fearless and precise manner to vital processes. He also embraced the other natural agents in his chain of conservation. In 1853 Mr. Waterston proposed, independently, the meteoric theory of the sun's heat, and in 1854 Professor William Thomson applied his admirable mathematical powers to the development of the theory; but six years previously the subject had been handled in a masterly manner by Mayer, and all that I have said on this subject has been derived from him. When we consider the circumstances of Mayer's life, and the period at which he wrote, we cannot fail to be struck with astonishment at what he has accomplished. Here was a man of genius working in silence, animated solely by a love of his subject, and arriving at the most important results, some time in advance of those whose lives were entirely devoted to Natural Philosophy. It was the accident of bleeding a feverish patient at Java in 1840 that led Mayer to speculate on these subjects. He noticed that the venous blood in the tropics was of a much brighter red than in colder latitudes, and his reasoning on this fact led him into the laboratory of natural forces, where he has worked with such signal ability and success. Well, you will desire to know what has become of this man. His mind gave way; he became insane, and he was sent to a lunatic asylum. In a biographical dictionary of his country it is stated that he died there: but this is incorrect. He recovered; and, I believe, is at this moment a cultivator of vineyards in Heilbronn.

June 20th, 1862.

While preparing for publication my last course of lectures on Heat, I wished to make myself acquainted with all that Mayer had

done in connection with this subject. I accordingly wrote to two gentlemen who above all others seemed likely to give me the information which I needed. Both of them are Germans, and both particularly distinguished in connection with the Dynamical Theory of Heat. Each of them kindly furnished me with the list of Mayer's publications, and one of them was so friendly as to order them from a bookseller, and to send them to me. This friend, in his reply to my first letter regarding Mayer, stated his belief that I should not find anything very important in Mayer's writings; but before forwarding the memoirs to me he read them himself. His letter accompanying the first of these papers, contains the following words:—"I must here retract the statement in my last letter, that you would not find much matter of importance in Mayer's writings: I am astonished at the multitude of beautiful and correct thoughts which they contain;" and he goes on to point out various important subjects, in the treatment of which Mayer had anticipated other eminent writers. My second friend, in whose own publications the name of Mayer repeatedly occurs, and whose papers containing these references were translated some years ago by myself, was, on the 10th of last month, unacquainted with the thoughtful and beautiful essay of Mayer's, entitled "*Beiträge zur Dynamik des Himmels*;" and in 1854, when Professor William Thomson developed in so striking a manner the meteoric theory of the sun's heat, he was certainly not aware of the existence of that essay, though from a recent article in '*Macmillan's Magazine*' I infer that he is now aware of it. Mayer's physiological writings have been referred to by physiologists—by Dr. Carpenter, for example—in terms of honourable recognition. We have hitherto, indeed, obtained fragmentary glimpses of the man, partly from physicists and partly from physiologists; but his total merit has never yet been recognized as it assuredly would have been had he chosen a happier mode of publication. I do not think a greater disservice could be done to a man of science, than to overstate his claims: such overstatement is sure to recoil to the disadvantage of him in whose interest it is made. But when Mayer's opportunities, achievements, and fate are taken into account, I do not think that I shall be deeply blamed for attempting to place him in that honourable position, which I believe to be his due.

Here, however, are the titles of Mayer's papers, the perusal of which will correct any error of judgment into which I may have fallen regarding their author. "*Bemerkungen über die Kräfte der unbelebten Natur*," Liebig's *Annalen*, 1842, Vol. 42, p. 231; "*Die Organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel*," Heilbronn, 1845; "*Beiträge zur Dynamik des Himmels*," Heilbronn, 1848; "*Bemerkungen über das Mechanische Equivalent der Wärme*," Heilbronn, 1851.

[J. T.]

Friday, January 23, 1863.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. Vice-President,
in the Chair.

JOHN TYNDALL, Esq. F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION.

On Radiation through the Earth's Atmosphere.

Nobody ever obtained the idea of a line from Euclid's definition that it is length without breadth. The idea is obtained from a real physical line drawn by a pen or pencil, and therefore possessing width; the idea being afterwards brought, by a process of abstraction, more nearly into accordance with the conditions of the definition. So also with regard to physical phenomena; we must help ourselves to a conception of the invisible by means of proper images derived from the visible, afterwards, purifying our conceptions to the needful extent. Definiteness of conceptions, even though at some expense to delicacy, is of the greatest utility in dealing with physical phenomena. Indeed it may be questioned whether a mind trained in physical research, can at all enjoy peace, without having made clear to itself some possible way of conceiving of those operations which lie beyond the boundaries of sense, and in which sensible phenomena originate.

When we speak of radiation through the atmosphere, we ought to be able to affix definite physical ideas, both to the term atmosphere and the term radiation. It is well known that our atmosphere is mainly composed of the two elements, oxygen and nitrogen. These elementary atoms may be figured as small spheres scattered thickly in the space which immediately surrounds the earth. They constitute about 99½ per cent. of the atmosphere. Mixed with these atoms we have others of a totally different character; we have the molecules, or atomic groups, of carbonic acid, of ammonia, and of aqueous vapour. In these substances diverse atoms have coalesced to form little systems of atoms. The molecule of aqueous vapour, for example, consists of two atoms of hydrogen united to one of oxygen; and they mingle as little triads among the monads of oxygen and nitrogen, which constitute the great mass of the atmosphere.

These atoms and molecules are separate; but in what sense? They are separate from each other in the sense in which the individual fishes

of a shoal are separate. The shoal of fish is embraced by a common medium, which connects the different members of the shoal, and renders intercommunication between them possible. A medium also embraces our atoms; within our atmosphere exists a second, and a finer atmosphere, in which the atoms of oxygen and nitrogen hang like suspended grains. This finer atmosphere unites not only atom with atom, but star with star; and the light of all suns, and of all stars, is in reality a kind of music propagated through this interstellar air. This image must be clearly seized, and then we have to advance a step. We must not only figure our atoms suspended in this medium, but we must figure them vibrating in it. In this motion of the atoms consists what we call their heat. "What is heat in us," as Locke has perfectly expressed it, "is in the body heated nothing but motion." Well, we must figure this motion communicated to the medium in which the atoms swing, and sent in ripples through it with inconceivable velocity to the bounds of space. Motion in this form, unconnected with ordinary matter, but speeding through the interstellar medium, receives the name of Radiant Heat; and, if competent to excite the nerves of vision, we call it Light.

Aqueous vapour was defined to be an invisible gas. Vapour was permitted to issue horizontally with considerable force from a tube connected with a small boiler. The track of the cloud of condensed steam was vividly illuminated by the electric light. What was seen, however, was not vapour, but vapour condensed to water. Beyond the visible end of the jet the cloud resolved itself into true vapour. A lamp was placed under the jet at various points; the cloud was cut sharply off at that point, and when the flame was placed near the efflux orifice the cloud entirely disappeared. The heat of the lamp completely prevented precipitation. This same vapour was condensed and congealed on the surface of a vessel containing a freezing mixture, from which it was scraped in quantities sufficient to form a small snowball. The beam of the electric lamp, moreover, was sent through a large receiver placed on an air-pump. A single stroke of the pump caused the precipitation of the aqueous vapour within, which became beautifully illuminated by the beam; while, upon a screen behind, a richly-coloured halo due to diffraction by the little cloud within the receiver flashed forth.

The waves of heat speed from our earth through our atmosphere towards space. These waves dash in their passage against the atoms of oxygen and nitrogen, and against the molecules of aqueous vapour. Thinly scattered as these latter are, we might naturally think meanly of them as barriers to the waves of heat. We might imagine that the wide spaces between the vapour molecules would be an open door for the passage of the undulations; and that if those waves were at all intercepted, it would be by the substances which form $99\frac{1}{2}$ per cent. of the whole atmosphere. Three or four years ago, however, it was found by the speaker that this small modicum of aqueous vapour intercepted fifteen times the quantity of heat stopped by the whole of the air in which

it was diffused. It was afterwards found that the dry air then experimented with was not perfectly pure, and that the purer the air became the more it approached the character of a vacuum, and the greater, by comparison, became the action of the aqueous vapour. The vapour was found to act with 30, 40, 50, 60, 70 times the energy of the air in which it was diffused; and no doubt was entertained that the aqueous vapour of the air which filled the Royal Institution theatre, during the delivery of the discourse, absorbed 90 or 100 times the quantity of radiant heat which was absorbed by the main body of the air of the room.

Looking at the single atoms, for every 200 of oxygen and nitrogen there is about 1 of aqueous vapour. This 1, then, is 80 times more powerful than the 200; and hence, comparing a single atom of oxygen or nitrogen with a single atom of aqueous vapour, we may infer that the action of the latter is 16,000 times that of the former. This was a very astonishing result, and it naturally excited opposition, based on the philosophic reluctance to accept a result so grave in consequences before testing it to the uttermost. From such opposition a discovery, if it be worth the name, emerges with its fibre strengthened; as the human character gathers force from the healthy antagonisms of active life. It was urged, that the result was on the face of it improbable; that there were, moreover, many ways of accounting for it, without ascribing so enormous a comparative action to aqueous vapour. For example, the cylinder which contained the air in which these experiments were made, was stopped at its ends by plates of rocksalt, on account of their transparency to radiant heat. Rocksalt is hygroscopic; it attracts the moisture of the atmosphere. Thus, a layer of brine readily forms on the surface of a plate of rock-salt; and it is well known that brine is very impervious to the rays of heat. Illuminating a polished plate of salt by the electric lamp, and casting, by means of a lens, a magnified image of the plate upon a screen, the speaker breathed through a tube for a moment on the salt; brilliant colours of thin plates (soap-bubble colours) flashed forth immediately upon the screen — these being caused by the film of moisture which overspread the salt. Such a film, it was contended, is formed when undried air is sent into the cylinder; it was, therefore, the absorption of a layer of brine which was measured, instead of the absorption of aqueous vapour.

This objection was met in two ways. Firstly, by showing that the plates of salt when subjected to the strictest examination show no trace of a film of moisture. Secondly, by abolishing the plates of salt altogether, and obtaining the same results in a cylinder open at both ends.

It was next surmised, that the effect was due to the impurity of the London air, and the suspended carbon particles were pointed to as the cause of the opacity to radiant heat. This objection was met by bringing air from Hyde Park, Hampstead Heath, Primrose Hill, Epsom Downs, a field near Newport in the Isle of Wight, St. Catharine's Down,

and the sea-beach near Black Gang Chine. The aqueous vapour of the air from these localities intercepted at least seventy times the amount of radiant heat absorbed by the air in which the vapour was diffused. Experiments made with smoky air proved that the suspended smoke of the atmosphere of West London, even when an east wind pours over it the smoke of the city, exerts only a fraction of the destructive powers exercised by the transparent and impalpable aqueous vapour diffused in the air.

The cylinder which contained the air through which the calorific rays passed was polished within, and the rays which struck the interior surface were reflected from it to the thermo-electric pile which measured the radiation. The following objection was raised:—You permit moist air to enter your cylinder; a portion of this moisture is condensed as a liquid film upon the interior surface of your tube; its reflective power is thereby diminished; less heat therefore reaches the pile, and you incorrectly ascribe to the absorption of aqueous vapour an effect which is really due to diminished reflection of the interior surface of your cylinder.

But why should the aqueous vapour so condense? The tube within is warmer than the air without, and against its inner surface the rays of heat are impinging. There can be no tendency to condensation under such circumstances. Further, let five inches of undried air be sent into the tube—that is, one-sixth of the amount which it can contain. These five inches produce their proportionate absorption. The driest day, on the driest portion of the earth's surface, would make no approach to the dryness of our cylinder when it contains only five inches of air. Make it 10, 15, 20, 25, 30 inches: you obtain an absorption exactly proportional to the quantity of vapour present. It is next to a physical impossibility that this could be the case if the effect were due to condensation. But lest a doubt should linger in the mind, not only were the plates of rock-salt abolished, but the cylinder itself was dispensed with. Humid air was displaced by dry, and dry air by humid in the free atmosphere; the absorption of the aqueous vapour was here manifest, as in all the other cases.

No doubt, therefore, can exist of the extraordinary opacity of this substance to the rays of obscure heat; and particularly such rays as are emitted by the earth after it has been warmed by the sun. It is perfectly certain that more than ten per cent. of the terrestrial radiation from the soil of England is stopped within ten feet of the surface of the soil. This one fact is sufficient to show the immense influence which this newly-discovered property of aqueous vapours must exert on the phenomena of meteorology.

This aqueous vapour is a blanket more necessary to the vegetable life of England than clothing is to man. Remove for a single summer-night the aqueous vapour from the air which overspreads this country, and you would assuredly destroy every plant capable of being destroyed by a freezing temperature. The warmth of our fields and gardens would pour itself unrequited into space, and the sun would

rise upon an island held fast in the iron grip of frost. The aqueous vapour constitutes a local dam, by which the temperature at the earth's surface is deepened: the dam, however, finally overflows, and we give to space all that we receive from the sun.

The sun raises the vapours of the equatorial ocean; they rise, but for a time a vapour screen spreads above and around them. But the higher they rise, the more they come into the presence of pure space; and when, by their levity, they have penetrated the vapour screen, which lies close to the earth's surface, what must occur?

It has been said that, compared atom for atom, the absorption of an atom of aqueous vapour is 16,000 times that of air. Now the power to absorb and the power to radiate are perfectly reciprocal and proportional. The atom of aqueous vapour will therefore radiate with 16,000 times the energy of an atom of air. Imagine then this powerful radiant in the presence of space, and with no screen above it to check its radiation. Into space it pours its heat, chills itself, condenses, and the tropical torrents are the consequence. The expansion of the air, no doubt, also refrigerates it; but in accounting for those deluges, the chilling of the vapour by its own radiation must play a most important part. The rain quits the ocean as vapour; it returns to it as water. How are the vast stores of heat set free by the change from the vaporous to the liquid condition disposed of? Doubtless in great part they are wasted by radiation into space. Similar remarks apply to the cumuli of our latitudes. The warmed air, charged with vapour, rises in columns, so as to penetrate the vapour screen which hugs the earth; in the presence of space, the head of each pillar wastes its heat by radiation, condenses to a cumulus, which constitutes the visible capital of an invisible column of saturated air.

Numberless other meteorological phenomena receive their solution, by reference to the radiant and absorbent properties of aqueous vapour. It is the absence of this screen, and the consequent copious waste of heat, that causes mountains to be so much chilled when the sun is withdrawn. Its absence in Central Asia renders the winter there almost unendurable; in Sahara the dryness of the air is sometimes such, that though during the day "the soil is fire and the wind is flame," the chill at night is painful to bear. In Australia, also, the thermometric range is enormous, on account of the absence of this qualifying agent. A clear day, and a dry day, moreover, are very different things. The atmosphere may possess great visual clearness, while it is charged with aqueous vapour, and on such occasions great chilling cannot occur by terrestrial radiation. Sir John Leslie and others have been perplexed by the varying indications of their instruments on days equally bright—but all these anomalies are completely accounted for by reference to this newly-discovered property of transparent aqueous vapour. Its presence would check the earth's loss; its absence, without sensibly altering the transparency of the air, would open wide a door for the escape of the earth's heat into infinitude.

[J. T.]

Friday, March 6, 1863.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. Vice-President,
in the Chair.

WILLIAM ALLEN MILLER, M.D. Treasurer R.S.

On the Photographic Transparency of Bodies, and on the Photographic Spectra of the Elementary Bodies.

AFTER a few preliminary remarks upon the triple nature of the force, *calorific, luminous, and chemical*, associated together in the radiation which emanates from luminous sources, the speaker stated his intention of limiting himself in great measure to some recent investigations upon the chemical rays.*

It is well known that bodies which are transparent to light are not equally so to radiant heat. Glass, for example, which to the eye is perfectly transparent and limpid, arrests a large portion of the rays of heat emitted by bodies which are not sufficiently hot to become luminous. Pure rock-salt, on the other hand, transmits rays of both light and heat from all sources freely. In like manner, in the case of rays which produce chemical action, corresponding effects have been observed; glass absorbing many of the chemical rays, whilst quartz transmits such rays freely.

The chemical rays emitted by luminous objects vary greatly both in quantity and in quality, some sources of light emitting rays of much higher refrangibility than others. Thus, the flame of ordinary coal-gas burned in admixture with air, so as to produce the blue light of a smokeless gas flame, gives out scarcely any rays capable of affecting an ordinary photographic plate; whilst the same amount of gas, burned

* The expressions here employed are simply used as descriptive of the effects ordinarily produced by the different portions of the spectrum, not as necessarily implying that the rays which produce the effects of heat, light, and chemical action respectively are essentially different, except in the number and frequency of the vibrations by which they are produced, the most refrangible rays being produced by the shortest and most frequent vibrations.

in the ordinary manner for illumination, emits a very decided though limited amount of rays capable of producing chemical action. The rays emanating from the intensely hot jet of the oxyhydrogen flame, are nearly without action upon a sensitive surface of collodion; whilst if thrown upon a ball of lime, the light then emitted contains as large a proportion of chemical rays as the solar light, and of very nearly the same refrangibility. But the most remarkable source of the chemical rays is afforded by the light of the electric spark or of the voltaic arc, the chemical spectrum of which is three or four times as long as the chemical spectrum obtainable from the sun itself.

1. *Photographic Transparency of various Media.*—Amongst the methods of testing the extent of chemical action of any given radiant source, the most convenient is that which is dependent upon the extent of photographic effect exerted upon a surface of collodion coated with iodide of silver, on which the spectrum is allowed to fall.

In no case does it appear that any non-luminous source can emit chemical rays of sufficient intensity to traverse ordinary refracting media; and amongst the rays given off by various luminous objects, it is found that the chemical effects upon the collodion plate are not perceptible in those portions upon which the first three-fourths of the visible spectrum has fallen, but they commence powerfully in the last fourth; and in the case of the electric spark are prolonged to an extent equal to between four and five times the length of the visible portion.

A diagram exhibiting the relative lengths of the visible solar spectrum, and the photographic spectrum obtained from the electric spark between silver points, showed this fact in a striking manner.

It was known to those who have studied the spectrum, that many colourless substances besides glass exert an absorptive action upon some of these chemical rays; but the subject had not hitherto received that careful experimental examination which its importance seems to warrant. Exact knowledge upon these points became requisite in the course of an investigation upon the photographic spectra of the metals in which the speaker was engaged.

In the prosecution of these inquiries it was a desideratum to procure some substance which should possess a higher dispersive power than quartz, and which, whilst avoiding the double refraction of quartz, should yet allow the free passage of the chemical rays. The speaker was hence led to try a variety of substances which, owing to their transparency to light, might reasonably be hoped to possess chemical transparency also.

The inquiry soon extended itself beyond the limits originally proposed, and ultimately embraced a large number of bodies in the solid, liquid, and gaseous conditions.

Before, however, proceeding to detail the results obtained, the speaker alluded to the discovery of Professor Stokes, that many bodies exist which, when placed in the invisible extra-violet, or more refrangible portion of the spectrum, exhibit the remarkable power of absorbing

the rays of this portion, and radiating them forth again in a visible form. Bodies which possess this power Mr. Stokes termed *fluorescent*. Now the chemical rays are exactly those which occasion fluorescence most powerfully. The light thus rendered visible is, however, very feeble, compared with that of the ordinary luminous portions; yet it may be rendered distinctly visible to those who are sufficiently near by allowing this chemically active portion of the spectrum to fall upon a screen consisting of some fluorescent substance, such as one of the salts of uranium, or a solution of *æsculin*, which latter was the material employed on the present occasion.

Returning then to the course of his own experiments, the speaker stated that although he had found in rock-salt, fluor-spar, water, and some few other substances, compounds which were almost as *diactinic*, or chemically transparent as quartz, he had not succeeded in finding anything which could be advantageously substituted for quartz in the preparation of the prisms and lenses required in the investigations in which he was engaged.

Among the most remarkable results upon the photographic transparency of bodies which had been observed by the speaker in his researches, were the following :—

1. Colourless solids which are equally transparent to the visible rays, vary greatly in permeability to the chemical rays.
2. Bodies which are photographically transparent in the solid form, preserve their transparency in the liquid and in the gaseous states.
3. Colourless transparent solids which absorb the photographic rays, preserve their absorptive action with greater or less intensity both in the liquid and in the gaseous states.
4. Pure water is photographically transparent, so that many compounds which cannot be obtained in the solid form sufficiently transparent for experiments, may be subjected to trial in solution in water.

The mode in which the experiments were conducted was the following :—

The source of light employed was the electric spark obtained between two metallic wires, generally of fine silver, connected with the terminals of the secondary wires of an induction coil, into the primary circuit of which was introduced a condenser, and into the secondary circuit a small Leyden jar. The light of the sparks was then allowed to fall upon a vertical slit, either before or after traversing a slice or stratum of the material of which the electric transparency was to be tested; the transmitted light was then passed through a quartz prism, placed at the angle of minimum deviation. Immediately behind this was a lens of rock crystal, and behind this at a suitable distance the spectrum was received upon the sensitive surface of collodion. Liquids were contained in a small glass cell with quartz faces, and gases and vapours in long tubes closed at their extremities with thin

plates of polished quartz. The following tables exhibiting the relative diactinic power of various solids, liquids, and gases and vapours were then commented on.

PHOTOGRAPHIC TRANSPARENCY OF

SOLIDS.		LIQUIDS.		GASES AND VAPOURS.	
Rock Crystal	74	Water	74	Oxygen	74
Ice	74	Alcohol	63	Nitrogen	74
Fluor Spar	74	Chloroform	26	Hydrogen	74
Topaz	65	Benzol	21	Carbonic Acid	74
Rock Salt	63	Wood Spirit	20	Olefiant Gas	66
Iceland Spar	63	Ether	16	Marsh Gas	63
Sulphate of Magnesia	62	Acetic Acid	16	Coal Gas	37
Borax	62	Oil of Turpentine	8	Benzol Vapour	35
Diamond	62	Bisulphide of Carbon	6	Hydrochloric Acid	55
Bromide of Potassium	48			Hydrobromic Acid	23
Thin Glass	20			Hydriodic Acid	15
Iodide of Potassium	18			Sulphurous Acid	14
Mica	18			Sulphuretted Hydrogen	14
Nitrate of Potash	16				

The effects indicated in these tables were rendered visible to the audience, by illuminating the photographic negative of the electric sparks from silver points by the light of the electric lamp, and bringing the image to a distinct focus upon the screen by means of a condensing lens.

Each photograph was obtained under circumstances varying only in the nature of the transparent medium through which the rays of the spark from the silver points were made to pass, before they were allowed to fall upon the collodion plate. When absorption occurs, it is almost always exhibited upon the most refrangible rays; but in the case of the coloured gases and vapours, chlorine, bromine, and iodine, the absorption differs from the general rule, and is by no means proportioned to the depth of colour. A column of chlorine with its yellowish green colour cuts off the rays of the *less* refrangible extremity through fully two-thirds of the spectrum; the red vapour of bromine cuts off about one-sixth of the length of the spectrum, the absorbent action being limited to the less refrangible extremity, whilst the deep violet-coloured vapours of iodine allow the less refrangible rays to pass freely for the first fourth of the spectrum; then a considerable absorption occurs, and afterwards a feeble renewal of the photographic action is exhibited towards the more refrangible end.

Diactinic bases, when united with diactinic acids, usually furnished diactinic salts; but such a result was not uniformly observed; the silicates were none of them as transparent as silica itself in the form of rock crystal. Again, hydrogen is eminently diactinic, and iodine vapour, notwithstanding its deep violet colour, is also largely diactinic; but hydriodic acid gas is greatly inferior to either of them.

The same substance, however, whatever may be its physical form, whether solid, liquid, or gaseous, preserves its character; no chemically opaque solid, though transparent to light, becoming transparent pho-

tographically by liquefaction or volatilization; and no transparent, solid being rendered chemically opaque by change of form. Hence it is obvious that this opacity or transparency is intimately connected with the atomic or chemical character of the body, and not merely with its state of aggregation. Although the absorption of the chemical rays varies greatly in the different gases, which therefore in this action display an analogy to their effects upon radiant heat, yet those gases which absorb the rays of heat most powerfully are often highly transparent to the chemical rays, as is seen in the case of aqueous vapour, of carbonic acid, cyanogen, and olefiant gas, all of which are compound substances, not chemical elements. These observations were considered by the speaker as opposed to the view of Dr. Tyndall, that compounds, *as compounds*, act more energetically than elementary bodies in absorbing the heat-rays, owing to the greater inertia of the particles of compounds. Any physical cause of this kind ought, however, to enable such bodies to act equally as absorbents of the luminous and chemical rays.

In the case of reflection from polished surfaces, the metals were found to vary in the quality of the rays reflected; gold and lead, although not the most brilliant, reflecting the rays more uniformly than the brilliant white surfaces of silver and speculum metal.

2. *Photographic Spectra of the Elements.*—But the most interesting results are those obtained by examining the spectra produced by varying the nature of the metallic electrodes employed as terminals to the secondary wires of the induction coil. Professor Wheatstone showed twenty-eight years ago that the visible spectrum of each metal is perfectly characteristic when electromagnetic sparks are transmitted between two surfaces of the metal; and the same thing is equally true of the invisible portion of the spectrum.

Even the various gaseous media become so intensely heated by the passage of the electric spark, that they furnish photographic spectra, each of which is characteristic of the body which occasions it; and when the electric discharge of the secondary coil becomes intensified by use of the Leyden jar, the sparks not only produce the spectra due to the metals, but to the gaseous medium in which the electrodes are immersed; so that a mixed spectrum is the result. The spectra produced by the metals are characterized by bands, of which the extremities only are visible; whilst the gaseous spectra yield continuous lines, which traverse the whole width of the spectrum. When a compound gas is made the medium of the electric discharge, the spectra produced are those of the elementary components of the gas. It seems as though at these intense temperatures chemical combinations were impossible; and oxygen and hydrogen, chlorine and the metals probably might all coexist in a separate form, though mechanically intermingled.

The spectrum produced by the ignition of a solid or a liquid always yields a continuous band of light, containing rays of all degrees

of refrangibility; but the same body, when converted into vapour, usually produces a spectrum consisting of a series of bright bands of particular colours, separated from each other by intervals more or less completely dark, gaseous bodies emitting rays of certain definite refrangibility only.

From the striped character of the photographic spectra, it is obvious that the vibrations are emitted from the different metals in the form of vapour, and not merely in that of detached particles projected from the electrodes by disruptive discharge.

This observation may give some idea of the intensely high temperature attained by the spark; since it is uniformly observed that the higher the temperature, the more refrangible are the vibrations. We are, indeed, furnished in this case with a rude, but still, under the circumstances, with a valuable pyrometric means of estimating these exalted temperatures.

To give an illustration of the mode of applying the observation: The hottest wind-furnace of ordinary construction yields a temperature probably not much exceeding $4,500^{\circ}$ F. By calculations founded upon the amount of heat ascertained by Andrews and others to be emitted during the combustion of a given weight of hydrogen, and the experiments of Regnault upon the specific heat of oxygen, hydrogen, and steam, it has been shown by Bunsen that the temperature of the oxyhydrogen flame cannot exceed $14,580^{\circ}$ F. The speaker stated that he had obtained spectra by introducing lime and sulphate of magnesia into the oxyhydrogen jet: these incombustible materials therefore could not be heated by the burning gases to a higher point than $14,600^{\circ}$ F., but the spectra so obtained coincide in their photographic lengths with that of the solar spectrum. Hence, the temperature of the sun may be approximatively estimated to be not higher than that of the oxyhydrogen flame. It certainly appears to be far below that of the electric spark. Magnesium in the electric spark gives a remarkably strong band just beyond the limits of the solar spectrum. Now magnesium is as clearly proved to exist in the solar atmosphere as any element, if we be admitted to have any such proof at all. But inasmuch as the special band which characterizes magnesium at a high temperature in the electric spark is wanting in the solar spectrum, it is difficult to avoid the conclusion that the temperature of the solar atmosphere is below that generated by the electric spark.

The speaker then adverted to Kirchhoff's well-known theory of the origin of Fraunhofer's dark lines in the solar spectrum, based on the observation that when any substance is heated or rendered luminous, rays of a definite refrangibility are given out by it; whilst the same substance, vapour, or gas, has the power of absorbing rays of this identical refrangibility. Now Kirchhoff supposes that in the luminous atmosphere of the sun the vapours of various metals are present, each of which would give its characteristic system of bright lines; but behind this incandescent atmosphere containing metallic vapour is the still more intensely heated solid or liquid nucleus of the sun,

which emits a brilliant continuous spectrum, containing rays of all degrees of refrangibility. When the light of this intensely heated nucleus is transmitted through the incandescent photosphere of the sun, the bright lines which would be produced by the photosphere are *reversed*, and Fraunhofer's black lines are therefore the reversed bright lines of which the spectrum due to the gaseous atmosphere of the sun would consist if the intensely heated nucleus were no longer there.

From his observations Kirchhoff concluded, by a comparison of the bright lines in the spectra of various metals with the dark lines of the solar spectrum, that potassium, sodium, magnesium, calcium, iron, nickel, chromium, manganese, and possibly cobalt, were present in the sun's atmosphere; and Ångström, continuing the examination into the blue and violet extremity, believes that he has shown the existence of hydrogen, aluminum, and possibly of strontium and barium. Diagrams were exhibited, showing some of the solar lines according to the observations of Kirchhoff and Ångström, from which it appears that

B	corresponds to	potassium.
C	"	hydrogen.
D	"	sodium.
E	"	iron.
b	"	iron and magnesium.
F	"	strontium (?) and iron and hydrogen.
G	"	iron.
H	"	calcium.

These observations on the solar spectrum give great interest to similar observations upon the stars, the light of which, however, is so feeble as to render the investigation of their spectra a task of no ordinary delicacy. Fraunhofer examined four or five of the brightest stars, and considered that the light of Sirius and Castor had lines differing decidedly from that of the sun. Capella and α Orionis resembled the solar light more closely. Donati has since examined several of the brighter stars, and given a drawing of some of their lines. The speaker had recently, conjointly with his friend, Mr. Huggins, been pursuing the same investigation with the excellent eight-inch equatorial refractor of the latter, and they had obtained some interesting results, having measured the principal lines in Sirius, Betelgeus, and Aldebaran, a diagram of which was exhibited, showing a more detailed spectrum of each of these stars than had been given by any previous observer. He also projected on the screen a microscopic photograph of the spectrum of Sirius that his friend and he had succeeded in obtaining. The light of this star, from the measurements of Sir J. Herschel and Mr. Bond, is little more than the one-six-thousandth-millionth part of that of the sun; and although probably not less in size than sixty of our suns, is estimated at the inconceivable distance of more than one hundred and thirty millions of

millions of miles. And yet it is influencing, in a measure, the chemical changes which are perpetually occurring upon the earth's surface, and by suitable means the changes may be recorded, estimated, and measured—the force which was registered by the photograph having emanated from Sirius twenty-one years before !

Capella, which Admiral Smyth estimates at more than three times the distance of Sirius, also gave a photograph, when its spectrum was thrown upon a collodion plate, the effect being produced by rays, which left the star probably when the oldest person in the room was yet a boy.

[W. A. M.]

Friday, March 20, 1863.

MAJOR-GENERAL EDWARD SABINE, R.A. President R.S. D.C.L.
Vice-President, in the Chair.

BALFOUR STEWART, Esq. F.R.S.

On the Forces concerned in producing Magnetic Disturbances.

WHEN a bar of steel has been magnetized, it has acquired a tendency to assume a definite position with respect to the Earth. Nothing is more widely known than this important fact, but at the same time there is nothing in Science more mysterious than its cause. We may endeavour to explain it by asserting that the Earth acts as a magnet; but whence it has acquired this magnetism, how it is distributed, and what are the causes of its many changes, are amongst the most perplexing and the most important of those problems in physical science which are yet unsolved.

The force with which the Earth acts upon the needle is directive merely; that is to say, the needle is neither attracted nor repelled as a whole, but simply twisted round, and in this respect the Earth is similar to a very powerful magnet, the pole of which is placed at a great distance from the needle upon which it acts.

If we keep a magnetic bar constantly suspended in the same place, its position will be subject to many changes. In the first place, there is that change which goes on in the same direction for a great many years together, in virtue of which a needle suspended in this place 200 years ago would have assumed a position very much different from its present one. Secondly, there is a change of which the period is one year, and this is called the annual variation. Thirdly, there is a change of which the period is one day, and which is called the daily variation. Fourthly, there is a change which depends upon the moon's hour angle; and, fifthly, the needle is subject to sudden and abrupt changes in position, which are called magnetic storms and which form the subject of this discourse. During the prevalence of these unaccountable phenomena the needle is found to oscillate rapidly and capriciously backwards and forwards, being now on the one side and now on the other of its normal or undisturbed position.

It should be here remarked, that the Physicist regards the needle merely as a vane (similar to a wind-vane) which serves to render visible the direction and intensity of that mysterious force which operates through the Earth. It is really the Earth's magnetism with which he concerns himself.

Gauss, who has done so much to further the science of magnetism, showed, by means of a preconcerted system of observation, that magnetic storms affected the needle at Göttingen and at other stations in Europe at precisely the same moment of absolute time; and after the establishment of the colonial observatories, it was found by General Sabine that the needle was affected in Toronto at precisely the moment when it was disturbed at Göttingen. Nor is it too much to say, with our present knowledge, that these remarkable disturbances break out at the same moment over every portion of our globe.

Having thus shown that these phenomena are cosmical in their character, the next point of interest is their connection with the sun. This has been placed beyond doubt chiefly through the labours of General Sabine, who found at Toronto and elsewhere that magnetic disturbances obey a law of hours. Mr. Broun also showed the same thing from his observation of the needle at Makerstoun, in Scotland. It may be instructive to point out how this proof was deduced from the colonial observations; and to make the matter plain, let us refer to an imaginary case in the familiar science of meteorology. Suppose that, while an observer is watching his thermometer there is a sudden influx of cold weather, and that it is wished to estimate the influence of this upon the thermometer on a given day and at a given hour of that day, what must the observer do? He must endeavour to ascertain, by the best possible means, what indication the thermometer would have afforded at that specified day and hour had there been no cold weather. Comparing this with the actual height of the mercury, and deducting the one from the other, he would clearly obtain a measure of the effect of the cold weather upon the thermometer.

A similar course was pursued by General Sabine in discussing the colonial magnetic observations, with the object of deducing the laws of disturbances. It was first necessary to ascertain by the best possible means what position the magnet would have assumed at any particular day and hour, had there been no disturbance. Calling this the normal value, the next course was to group together as disturbed, all those positions of the magnet which differed from the normal by more than a certain small quantity. The necessity for this separation will become evident when it is remarked that the disturbed and the regular observations have different hourly turning-points, and obey very different laws. Thus a disentanglement was effected, which was accomplished by the employment of a separating value. The selection of this value is to some extent arbitrary, but it was shown by reference to a diagram that the disturbance law at Kew was virtually the same, whether this were deduced (in the case of the declination) from 95 days of principal disturbances or from all disturbed observations which differ from the normal by more than $3^{\circ}.3$. It was also shown from the same diagram that easterly disturbances prevail at Kew during certain hours of the day, and westerly disturbances at certain other hours, thus exhibiting a daily law, and showing that disturbances are therefore connected with our luminary.

There is, however, a more interesting and mysterious connection than this. Professor Schwabe, of Dessau, has now for nearly forty years been watching the disc of the sun, and recording the groups of spots which have been visible, and he finds that these have a period of maximum nearly every ten years, two of these periods being the years 1848, 1859. Now it was likewise found by General Sabine, that the aggregate value of magnetic disturbances at Toronto attained a maximum in 1848, nor was he slow to remark that this was also Schwabe's period of maximum sun-spots, and it was afterwards found, by observations made at Kew, that 1859 (another of Schwabe's years) was also a year of maximum magnetic disturbance. This fact is eminently suggestive, and brings us at once into the presence of some great cosmical bond, different from gravitation, adding at the same time additional interest as well as mystery to these perplexing phenomena.

These are the grounds on which we suspect the sun to be the agent which causes magnetic disturbances, but there is also some reason to believe that on one occasion our luminary was caught in the very act. On the first of September, 1859, two astronomers, Messrs. Carrington and Hodgson, were independently observing the sun's disc, which exhibited at that time a very large spot, when about a quarter-past eleven they noticed a very bright star of light suddenly break out over the spot and move with great velocity across the sun's surface.

On Mr. Carrington sending afterwards to Kew Observatory, at which place the position of the magnet is recorded continuously by photography, it was found that a magnetic disturbance had broken out at the very moment when this singular appearance had been observed.

The next point to be noticed is, that magnetic storms are always accompanied by auroræ and by earth currents. With regard to the latter of these phenomena, a single word of explanation may be necessary. Earth currents are currents of electricity which traverse the surface of our globe, a portion of which is caught up by the telegraphic wires, which are often thereby seriously disturbed in their communications. A table was then referred to, which showed that auroræ and earth currents have the same ten-yearly period as sun-spots and magnetic disturbances, so that a bond of union exists between those four phenomena.

The question next arises, What is the nature of this bond? Now, with respect to that which connects sun-spots with magnetic disturbances we can as yet form no conjecture; but we may, perhaps, venture an opinion regarding the nature of that which connects together magnetic disturbances, auroræ, and earth currents. And here we may remark that this latter bond is the more definitely determined of the two, since the three phenomena which it embraces *invariably occur together*.

In order to exhibit the evidence upon which this hypothesis rests, it is necessary to refer to what is done at the Kew Observatory.

By means of an apparatus arranged by the late Mr. Welsh, the values of the *components* of the earth's magnetism, are there recorded

continuously by photography. Now there is a proposition which goes by the name of the parallelopiped of forces and which asserts that if three forces acting at a point be represented in magnitude, and in direction by the three sides of a parallelopiped, the resultant of these forces shall be represented in magnitude and direction by the diagonal of the parallelopiped. These three forces may be said to be *components* of the whole resultant force, since the joint action of the three is the same as that of the resultant.

To refer to a familiar case, suppose that there is a gust of wind which (as sometimes happens) is not horizontal, but blows downwards in a slanting direction from above. How shall we estimate the direction and the force of this wind? Let us have three pressure plates, one north and south, one east and west, and one up and down; then the north and south component of the wind will be given by the first, the east and west component by the second, and the up and down component by the third, and exhibiting the indications of these three pressure-plates by the three sides of a parallelopiped, the diagonal will represent the wind in magnitude and in direction.

Something of this kind must be done, if it be wished to record the disturbing force which acts upon the needle. Let there be three magnets free to move, two in a horizontal plane and one in a vertical direction; one swinging freely and pointing to magnetic north and south, one compelled by torsion to point to magnetic east and west, and a third balanced on a knife-edge so as to move up and down; the first will be sensitive to an east and west disturbing force, which will act at right angles to it, and tend to twist it round; the second will on the other hand be affected by a north and south force, and the third by a vertical force. It is easily seen how these three magnets will take the place of the three pressure plates, in the meteorological problem. Now if a mirror be attached to each of these magnets, it may be made to reflect the image of a dot of light upon a sheet of photographic paper, so that a small motion of the mirror shall cause a large motion of the dot, and if the paper itself move by clock-work, in a direction at right angles to that in which the dot moves on the paper, the photographic impression of the dot during a day's motion of the paper will be a curved line, and this being obtained for each of the three magnets, we shall be able to record continuously the changes which are taking place in the three components of the earth's magnetic force.

Reference was then made to a diagram in which these three curved lines were exhibited for September 1-2, 1859; and it was seen that about four o'clock in the early morning of September 2, the three components of the earth's magnetism at Kew were simultaneously and abruptly disturbed, and were kept at one side of their normal or undisturbed positions for many hours. During this time there were vivid auroræ which extended over the greater part of the globe, and even to as low a latitude as Cuba, and strong earth currents were also observed by Mr. C. V. Walker, on the various telegraphic lines. These currents were

found to change their direction every two or three minutes, going alternately from positive to negative, and back again to positive. It is therefore evident that currents varying in this manner could not have been the *cause* of magnetic disturbances in which the needle was kept on one side of its nominal position for many hours. But the curves of magnetic disturbance further exhibit sharp peaks and hollows, or wavelets, superimposed upon the great disturbance wave, and these wavelets change their direction every two or three minutes, in which respect they are comparable with earth currents. May not these wavelets be connected with earth currents and auroræ, and may not this connection be of the following kind? A peak denotes a small but rapid change of the earth's magnetic force in one direction, and a hollow, a similar change in the opposite direction. Now in a Ruhmkorff's coil we have—1st, a soft iron core, with a current circulating round it; 2nd, an insulator round the current; 3rd, a secondary coil above the insulator, containing perhaps several miles of fine wire. In this arrangement we have a discharge between the terminals of the secondary coil every time contact with the primary current is made, and one of an opposite character every time this contact is broken.

But the chief use of the primary current is to reverse the magnetism of the iron core, and could we reverse this, or even change it rapidly without a primary current, we should have the same effect, that is to say, we should have a secondary current in one direction, when the magnetism of the core was rapidly increased, and one an opposite direction, when this was rapidly diminished. An experiment was made, showing the mode of action of the Ruhmkorff's coil, and the following comparison was instituted between this instrument and our earth. The body of our earth may be likened to the soft iron core of a Ruhmkorff's machine, in which one of the small curve-peaks already alluded to denotes a rapid change of magnetism in one direction, and a hollow, a change of the opposite character. The lower strata of the atmosphere again resemble the insulator of the Ruhmkorff's machine, and the upper and rarer strata, the secondary conductor; again, the crust of the earth being permeated with moisture, becomes a conductor, and may therefore also be likened to the secondary coil. Whenever therefore we have a curve-rise, that is to say, a sudden change of the earth's magnetism in one direction, we should have in the upper strata of the atmosphere and in the crust of the earth currents of one kind; and when we have a curve-fall or a sudden change of magnetism in the opposite direction, we should have similar currents of an opposite description.

It need hardly be remarked, that those currents which take place in the upper strata of the atmosphere will form auroræ, while those in the crust of the earth will constitute earth currents.

Now, if this be the nature of that connection which subsists between magnetic disturbances, earth currents, and auroræ, may we not extend our inquiries, and ask, "If the sun's action is able to create a terrestrial aurora, why may he not also create an aurora in his own atmosphere? It occurred independently to General Sabine, Professor

Challis, and the speaker, that the red flames visible during a total eclipse may, indeed, be solar auroræ. In support of this hypothesis it may be remarked that, during the late total eclipse in Spain, Mr. De la Rue, by means of the Kew photoheliograph, proved that these red flames belong to the sun, and that they extended in one case to the distance of 70,000 miles beyond his photosphere. But, considering the gravity of the sun, we are naturally unwilling to suppose that there can be any considerable amount of atmosphere at such a distance from his surface; and we are therefore induced to seek for an explanation of these red flames amongst those phenomena which require the smallest possible amount of atmosphere for their manifestation. Now the experiments of Mr. Cassiot, and the observed height of the terrestrial aurora alike convince us that this meteor will answer our requirements best. And besides this, the curved appearance of these red flames, and their high actinic power in virtue of which one of them, not visible to the eye, was photographed by Mr. De la Rue, are bonds of union between these and terrestrial auroræ.

It has been remarked by General Sabine, that an auroral outburst in the sun may perhaps be responded to simultaneously by the different planets. If this be true, our whole solar system would seem to thrill almost like a living being under the excitement of this mysterious force. It has been likewise found by Mr. Cassiot, that electricity cannot pass through a perfect vacuum, so that perhaps we have only to observe the greatest height attained by a terrestrial aurora and by a solar red flame, in order to be able to assign the limit, not only of our own atmosphere, but also of that of our luminary.

One other point remains to be noticed in connection with magnetic disturbances, and this is, that there appear to be two separate disturbing forces, nearly opposite in character, both connected with the sun, which act simultaneously upon the magnet; the position which the latter assumes being due to the combined effect of both. This has been shown to be true by General Sabine, who has observed that the curve which exhibits the daily range of the east component of the disturbing force, is in many places very different in character from that which exhibits the same for the west component. And this difference between the two curves is of one kind at one station, and of another kind at another station. This duality of the disturbing forces may also be observed directly in the Kew disturbance-curves. Here it was shown, by means of models, kindly constructed by Mr. Beckley, and also by reference to the parallelogram of forces, that whenever the corresponding peaks and hollows for the different components continue to bear a definite proportion to one another, these then denote the action of a disturbing force, varying in intensity, but always preserving the same type.

A set of curves were exhibited in which this proportion held, and in which the disturbing force, whose variations were denoted by the peaks and hollows, was one which affected the north and south component twice as much as the other two. It was then shown by reference to the normal line, or line of no disturbance, that there was also in action

at that time another disturbing force, which was not however of the same variable character as that which caused the peaks and hollows.

The attention of foreign men of science has been much directed to the problem of terrestrial magnetism, and five sets of magnetographs, similar to those in operation at the Kew Observatory, have been already procured by foreign governments. These, however, will be placed in the northern hemisphere, and it is to be desired that some of our colonies in the southern hemisphere may come forward in order that by the next epoch of maximum disturbance (1869), there may be such a network of magnetic observatories as may enable us to obtain the solution of this interesting and important problem.

[B. S.]

Friday, March 27, 1863.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. Vice-President,
in the Chair.

WILLIAM CROOKES, Esq. F.C.S.

On the Discovery of the metal Thallium.

THE speaker commenced by remarking that the discovery of a new metal was no novelty in this century. Since its commencement, our knowledge of the material world had been increased by the discovery of no less than thirty-two of these elements. Glancing rapidly at the already known metals, the speaker said that seven of these were known to the ancients, and we have no knowledge of their first discovery. We know, however, that many are found native, and the others are separated from their ores either by heat alone, or by heat and the simplest chemical agencies. Most of those of more recent discovery have been obtained by exclusively chemical means, the exceptions being those, the separation of which, by means of voltaic electricity, by Sir Humphry Davy, makes the commencement of this century a marked epoch in the history of chemistry, and also of the Royal Institution. From the time of Sir Humphry Davy no new method for the recognition of elements had been discovered until the researches of Bunsen and Kirchhoff gave to the scientific world a definite knowledge of analysis by means of the spectrum. By this means Bunsen discovered the two new alkali metals cæsium and rubidium, and by the same means the speaker was led to the discovery of Thallium.

The special history of the discovery of the last-named metal offered a curious parallelism to the discovery of selenium by Berzelius. The great Swedish chemist was engaged on the examination of a residue from a sulphuric acid manufactory, in which he was induced to suspect the presence of tellurium. A complete examination proved to him that tellurium was absent; but in the course of his experiments he succeeded in separating a new element, belonging to the sulphur group, to which he gave the name of Selenium. The speaker, three years ago, was occupied in the analysis of a similar residue from some sulphuric acid works at Tilkerode, also with a view to the separation of tellurium. The sulphuric acid residue had originally been placed at his disposal by Professor Hofmann, in the year 1850, for the purpose of extracting selenium from it. In the distillation of the crude

selenium, there was left behind in the retorts a residuum which a few chemical tests led the speaker to conclude contained tellurium. This was placed aside for further examination, and remained unnoticed until, in the beginning of the year 1861, it was re-examined with the object of preparing tellurium from it. Not succeeding in finding evidences of the presence of this metal by chemical means, the speaker had recourse to the recently discovered means of spectrum analysis; but instead of noticing the alternate bands of light and shade characteristic of tellurium, he was surprised to observe a single green line of remarkable brilliancy and intensity, which had hitherto been unnoticed, and was communicated by no known element which could have been present in the residuum. (The spectrum of thallium was here projected on the screen, a 40-cell Grove's battery being used to produce the electric arc. Its green band appeared perfectly sharp and brilliant upon a dark background.) Further researches soon proved to the speaker that he was, in fact, dealing with an entirely new element; but the quantity contained in the material under analysis was so minute that the complete isolation was a matter of great difficulty. In September, 1861,* however, and by means of the galvanic battery, he succeeded in precipitating the metal in a pure form; to which, in consequence of the green band it communicates to the spectrum and to flame, he had previously given the name of Thallium, from *θαλλός*, a bud, the colour of early vegetation most nearly resembling the shade of green it gives. The metal was shown to friends soon after isolation, and its nature freely communicated; but no formal publication of this fact was made until the opening of the International Exhibition, on May 1, 1862, where, in a case deposited some days before, were displayed several grains of the new body, labelled and described as a *heavy metal*.

By the kindness of numerous friends, among whom may be mentioned Dr. Thorntliwaite, Professor Chandon, and Mr. Peter Spence, and by the munificence of the Royal Society, he has since found more prolific sources of the metal, and been able to work on a large scale.

While speaking of the metallic nature of Thallium, the speaker noticed the fact of the observation of the green line and separation of the metal, on the 16th of May last, independently of himself, by a skilful Belgian chemist, M. Lamy, who deposited a specimen in the International Exhibition in June, 1862.

Thallium, the speaker said, belongs to the class of heavy metals. Its specific gravity is 11.9, very nearly that of lead. In colour it most resembles cadmium. It possesses considerable lustre, but quickly tarnishes in the atmosphere. It is very soft, being easily scratched by lead. It is very malleable, and the speaker showed that it could easily be forced into wire, several lumps welding together in the cold into one solid rod. It marks paper like plumbago, but the mark quickly disappears by the formation of a light-coloured oxide, being reproduced however by the action of a soluble sulphide. Thallium, the speaker

* At the Lecture, the year 1862 was given inadvertently for 1861.—[W. C.]

showed, was strongly diamagnetic, being only inferior to bismuth in this respect; and Dr. Matthiessen had determined that its power of conducting electricity was very near that of lead. It is precipitated from its solution by means of the battery in beautiful crystals, the form of which has not yet been determined; and when the experiment is carefully conducted, these spread over the dish in branches like a delicate seaweed, or with a stronger solution like the club-moss. The atomic weight of thallium is about 203. Its chemical properties are at first sight rather anomalous, and seem to justify the remark of M. Dumas, that it is the *ornithorhynchus* of metals. That eminent French chemist ranks it with the alkaline metals, with which indeed it has some common properties: but the speaker is inclined to class it with the heavy metals, lead and silver, to which it seems more nearly allied both by its chemical and physical properties: several of the chemical reactions of thallium were shown in corroboration of this view. Like the metals potassium and sodium, it forms a soluble oxide which is endowed with strongly alkaline properties; like them too it is not precipitated by sulphuretted hydrogen, nor in its lower state of oxidation by an alkali; it also forms an insoluble platino-chloride, but here the resemblance ceases. All the other reactions are similar to those of the heavy metals. The soluble oxide is in reality also allied to the oxides of silver and lead (which are both soluble in water and alkaline), inasmuch as it has no affinity for water, being rendered anhydrous even at the common temperature in a vacuum. It is readily precipitated in the metallic form from its saline solutions by zinc. It forms an insoluble peroxide, sulphide, iodide, bromide, chromate and sulphocyanide, and a slightly soluble protochloride, sesquichloride, ferrocyanide and chlorate.

The alloys of Thallium have not yet been much studied. The uses of the metal the speaker said he was hardly yet in a position to dwell upon. At present it would appear only to confirm in the most striking manner the value of spectrum analysis as a guide in chemical research. As an agent for the production of colour in pyrotechny, it was only equalled in brilliancy by the two other elements which give a monochromatic light, lithium and sodium. The speaker here illustrated the monochromatic nature of the thallium light, by throwing the magnified image of its electric arc on a bouquet of variously coloured flowers; and concluded by thanking the audience for the indulgence they had extended to his first public discourse.

[W. C.]

Friday, May 15, 1863.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. Vice-President,
in the Chair.

WILLIAM ODLING, Esq. M.B. F.R.S.

SECRETARY TO THE CHEMICAL SOCIETY.

On the Molecule of Water.

DOES the molecule of water consist of a single combining proportion of hydrogen having the relative weight 1, united with a single combining proportion of oxygen having the relative weight 8, or does it consist of two combining proportions of hydrogen having each the relative weight 1, united with a single combining proportion of oxygen having the relative weight 16? In other words, ought the molecule of water to be represented by the formula $\text{H}_2\text{O}'$, in which O' stands for 8 parts by weight of oxygen, or by the formula $\text{H}_2\text{O}''$, in which O'' stands for 16 parts by weight of oxygen? The question is entirely one of fact. If the hydrogen of water is experimentally indivisible, it necessarily constitutes an indivisible proportion, or atomic proportion, or so called atom. If, on the other hand, it is experimentally divisible into two equal parts, it must consist of at least two atomic proportions, or two atoms.

Similar questions with regard to the molecular formulæ of other compounds have been decided with an almost absolute unanimity. Thus chlorhydric or muriatic acid gas consists of 1 part by weight of hydrogen united with 35.5 parts by weight of chlorine, and its molecule is represented by the formula HCl , in which H stands for a single combining proportion of hydrogen, and Cl for a single combining proportion of chlorine, having 35½ times the weight of the single combining proportion of hydrogen.

Phosphoretted hydrogen gas consists of 1 part by weight of hydrogen united with 10.3 parts by weight of phosphorus. Its molecule, however, is not represented by the formula HP' , in which H stands for a single combining proportion of hydrogen having the relative weight 1, and P' for a single combining proportion of phosphorus having the

relative weight 10·3 ; but by the formula H_3P''' , in which H_3 stands for three separable proportions of hydrogen having each the relative weight 1, and P''' for a single indivisible proportion of phosphorus having the relative weight 31.

Silicated hydrogen gas consists of 1 part by weight of hydrogen united with 7 parts by weight of silicon. Its molecule, however, is not represented by the formula HSi' , in which Si' stands for 7 parts of silicon ; or yet by the formula H_7Si''' , in which Si''' stands for 21 parts of silicon, as the resemblance in properties between silicated and phosphoretted hydrogen might seem to require ; but by the formula H_4Si'''' , in which H_4 stands for four separable proportions of hydrogen having each the relative weight 1, while Si'''' stands for a single indivisible proportion of silicon having the relative weight 28.

Why then do chemists represent chlorhydric acid, and its prototype fluorhydric acid, as each containing one combining proportion of hydrogen ; phosphoretted hydrogen, and its prototype ammonia, as each containing three combining proportions of hydrogen ; and silicated hydrogen, and its prototype marsh-gas, as each containing four combining proportions of hydrogen ? For all are agreed that in the molecules of fluorhydric acid and chlorhydric acid there is one part of hydrogen united with 19 parts of fluorine, and 35·5 parts of chlorine respectively ; that in the molecules of ammonia and phosphoretted hydrogen there are three parts of hydrogen united with 14 parts of nitrogen, and 31 parts of phosphorus respectively ; and that in the molecules of marsh-gas and silicated hydrogen there are four parts of hydrogen united with 12 parts of carbon, and 28 parts of silicon respectively.

The reasons by which chemists are induced to be so seemingly inconsequent are very numerous, and some of them very recondite ; but the reason of greatest weight and most obvious character is drawn from the phenomena of substitution ; by which is meant the change effected in the composition of various bodies, by the abstraction of certain of their constituent elements, and the introduction of other elements or groupings in their stead. Thus we have sodium-alcohol, in which a portion of the hydrogen of common alcohol is substituted by its equivalent of sodium ; bromaniline, in which a portion of the hydrogen of common aniline is substituted by its equivalent of bromine ; nitrophenol, in which a portion of the hydrogen of common phenol is substituted by its equivalent of peroxide of nitrogen ; and so in very many other instances. Now the conclusions deducible from the phenomena of substitution, whether elementary or compound, direct or indirect, obvious or latent, are, as will presently appear, of the greatest importance in determining the molecular formulæ of compound bodies.

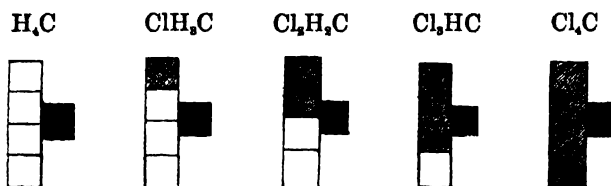
It is evident, for instance, that the molecule of marsh-gas must contain four proportions of hydrogen, because we are able to replace $\frac{1}{4}$, $\frac{2}{4}$, $\frac{3}{4}$, and $\frac{4}{4}$ of its hydrogen by four successive substitutions, to produce a series of bodies differing from one another by a regular gradation

of properties. Thus the final product of the action of chlorine upon marsh-gas is a compound in which all the hydrogen of marsh-gas is replaced by its equivalent of chlorine; so that if we consider the molecule of marsh-gas to consist of one combining proportion of hydrogen united with 3 parts by weight of carbon, then the molecule of its chloro-derivative will consist of one combining proportion of chlorine united with 3 parts by weight of carbon; or if we represent the molecule of marsh-gas to consist of four combining proportions of hydrogen united with 12 parts by weight of carbon, then the molecule of its chloro-derivative must consist of four combining proportions of chlorine united with 12 parts by weight of carbon; as shown in the following table:—

$C' = 3$		$C''' = 12$	
(3)	$H C'$	$H_4 C'''$	(12)
(12)	$3H C', Cl C'$	$Cl H_3 C'''$	(12)
(6)	$H C', Cl C'$	$Cl_2 H_2 C'''$	(12)
(12)	$H C', 3Cl C'$	$Cl_3 H C'''$	(12)
(3)	$Cl C'$	$Cl_4 C'''$	(12)

Now exactly intermediate between the original hydride and the final chloride is a body which contains hydrogen and chlorine in equivalent proportions, and which consequently cannot be represented with less than 6 parts by weight of carbon, and may of course be represented with 12 parts; while intermediate between this body and marsh-gas on the one hand, and between it and chloride of carbon on the other, are two additional compounds, the one containing three combining proportions of hydrogen and one combining proportion of chlorine, the other containing three combining proportions of chlorine and one combining proportion of hydrogen, and both of them consequently incapable of being represented with less than 12 parts by weight of carbon. There are thus two sets of formulæ presented for selection; the one representing the above-mentioned compounds with unequal quantities of carbon by the simplest possible individual formulæ, the other representing them with an equal quantity of constituent carbon by the simplest possible series of formulæ; and a little consideration has led chemists to the unanimous opinion that the formulæ in the second column do, while those in the first column do not, express the actual correlations of the bodies represented. For these bodies manifest in every respect a regular seriation of properties, such as necessarily would be the case if their molecules differed from one another only by a gradually increasing substitution of chlorine for hydrogen; but such as could not be the case if the constituent carbon of their respective molecules varied in the ratio of 3 to 12, or even of 3 to 6. Moreover, by adopting the formula $H_4 C'''$ for marsh-gas we perceive at once why in its chloro-derivatives the hydrogen and chlorine should always stand to one another in the relation of fourths. But if marsh-gas is to

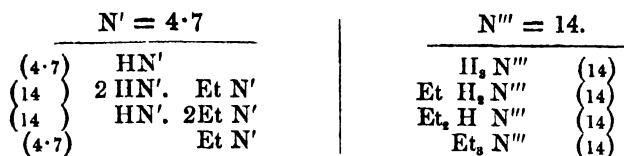
be represented by the formula HC' , and its chloro-derivatives regarded as compounds of HC' with ClC' , there is no reason why we should not have compounds of 1 HC' with 2 or 5, or 6 of ClC' , or of 2 HC' with 3 ClC' , &c., in which the hydrogen would stand to the chlorine in the relation of a third, or of two-thirds, or of a fifth, or of a sixth, &c. But with the tetrahydric model, corresponding to the formula H_4C''' , and represented below, the successive displacement of the hydrogen of marsh-gas in fourths, and in no other proportion, becomes perfectly intelligible, thus:—



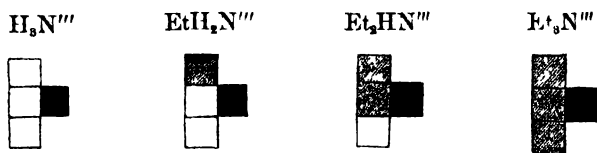
By the action of chlorine upon marsh-gas, there are produced in succession monochloromethene, in which one-fourth of the hydrogen is displaced by chlorine; then dichloromethene, in which two-fourths of the hydrogen are displaced by chlorine; then trichloromethene or chloroform, in which three-fourths of the hydrogen are displaced by chlorine; and lastly, tetrachloromethene, in which all four-fourths of the hydrogen are displaced by chlorine. If, then, we take 1 relative part of hydrogen as the least indivisible proportion of hydrogen that can enter into a combination, it is evident that the molecule of marsh-gas must contain four such parts, or four combining proportions, or, in common parlance, four atoms of hydrogen.

The case of ammonia is even more striking than that of marsh-gas. When we effect the substitution of some element or grouping for the hydrogen of ammonia, the substituted and remaining hydrogen are not related to one another in fourths, as happens with marsh-gas, but always in thirds. When, for instance, potassium is heated in ammonia, there is produced the compound known as potassamide, which consists of one combining proportion of potassium, and two combining proportions of hydrogen—one-third of potassium and two-thirds of hydrogen—united with 14 parts by weight of nitrogen. Again, when ammonia is acted on by iodine, there is produced the very explosive body known as diiodamide, which consists of two combining proportions of iodine and one combining proportion of hydrogen—two-thirds of iodine and one-third of hydrogen—united with 14 parts by weight of nitrogen. While if we act upon ammonia by excess of chlorine, we obtain the highly-dangerous compound known as trichloramide or chloride of nitrogen, in which all the hydrogen of ammonia is replaced by chlorine. But the most satisfactory evidence of the trihydric character of ammonia is afforded by Hofmann's experiments on the volatile organic

bases, in which he succeeded in replacing one-third, two-thirds, and three-thirds of the hydrogen of ammonia by one and the same radicle, and by a continuation of one and the same process ; thus :—



The composition of ethylamine and diethylamine cannot possibly be expressed with less than 14 parts by weight of nitrogen, whilst that of ammonia and triethylamine might be represented with one-third of that quantity, or with 4.7 parts. But all four bodies present such a marked resemblance to one another, and such a regular gradation of properties, as could not be the case unless their respective molecules were strictly comparable in constitution. For instance, their boiling points and vapour-densities increase by a regular progression, according to the regular increase in the substitution of ethyl for hydrogen ; whereas it is certain that the boiling point and vapour-density of a body containing but one proportion of ethyl and 4.7 parts of nitrogen would bear no relation of series to the boiling points and vapour-densities of bodies containing three proportions of conjoint hydrogen and ethyl united with 14 parts by weight of nitrogen. Moreover, by adopting the trihydric model for ammonia, corresponding to the formula $\text{H}_3\text{N}'''$, we perceive at once why the replacement of its hydrogen must take place in thirds ; or, in other words, why we cannot obtain ammonias in which one-half or one-fourth of the hydrogen is replaced by equivalent substitution, but only those in which one-third or two-thirds is so replaced ; thus :—



Again, ammonias are known to chemists in which two-thirds of the hydrogen are replaced by one, and the remaining third of hydrogen by some other radicle ; or in which all three thirds are replaced by three different radicles, as instanced by diethyl-phenylamine $\text{Et}_2\text{PhN}'''$, and methyl-ethyl-phenylamine $\text{MeEtPhN}'''$, for example.

Seeing, then, that there are bodies, such as marsh-gas and silicated hydrogen, of which the hydrogen is replaceable in fourths at four successive stages, and which are consequently represented with four atoms of hydrogen ; and that there are other bodies, such as ammonia

and phosphoretted hydrogen, of which the hydrogen is replaceable in thirds at three successive stages, and which are consequently represented with three atoms of hydrogen,—it is not *à priori* improbable that there may be yet other bodies, such as water and sulphydric acid, in which the hydrogen is replaceable in halves at two successive stages, and which must consequently be represented with two atoms of hydrogen. If we act upon water by excess of metallic sodium, for instance, we obtain the compound known as oxide of sodium; and accordingly as we represent the molecule of water by the formula HO' , in which O' stands for 8 parts by weight of oxygen, or by the formula $\text{H}_2\text{O}''$, in which O'' stands for 16 parts by weight of oxygen, so must we represent oxide of sodium by the formula NaO' , or $\text{Na}_2\text{O}''$; thus :—



But exactly intermediate between water and oxide of sodium, we have the very definite compound known as hydrate of sodium, which consists of one combining proportion of sodium and one combining proportion of hydrogen united with 16 parts by weight of oxygen, and which cannot be represented save with 16 parts by weight of oxygen. Now, just as the three intermediate bodies between marsh-gas and chloride of carbon force us to represent these extreme bodies with 12 parts of carbon, so that all five may appear as members of one and the same series; just as the two intermediate bodies between ammonia and triethylamine force us to represent these extreme bodies with 14 parts of nitrogen, so that all four may appear as members of one and the same series; so must the intermediate body between water and oxide of sodium force us to represent these extreme bodies with 16 parts of oxygen, so that all three may appear as members of one and the same series. If marsh-gas and chloride of carbon must be represented by formulæ similar to those of their intermediate bodies, and consequently with four atoms of hydrogen and chlorine respectively; if ammonia and triethylamine must be represented by formulæ analogous to those of their intermediate bodies, and consequently with three atoms of hydrogen and of chlorine respectively; then water and oxide of sodium must be represented by formulæ analogous to that of their intermediate body, and consequently with two atoms of hydrogen and of sodium respectively. In the intermediate carbon-bodies, the remaining and replaced hydrogen form fourths of the whole; in the intermediate nitrogen bodies, the remaining and replaced hydrogen form thirds of the whole; while in the intermediate oxygen compound, the remaining and replaced hydrogen form halves of the whole. Hence, if the molecule of marsh-gas is to be represented by a tetrahydric model, and

that of ammonia by a trihydric model, then the molecule of water must be represented by a dihydric model; thus:—

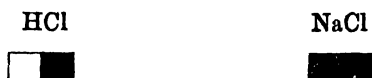


When water is acted upon by metallic sodium, one equivalent of sodium expels one equivalent of hydrogen, to form the well-defined body hydrate of sodium, and the action proceeds no further. But when melted hydrate of sodium, at a temperature approaching that of redness, is acted upon by metallic sodium, a second equivalent of sodium turns out a second equivalent of hydrogen, to produce oxide of sodium. This replacement of the hydrogen of water by sodium at two successive stages,—the first stage of replacement taking place at ordinary temperatures, and the second stage at a dull red heat,—is even better defined than the replacement of the hydrogen of ammonia by ethyl at three successive stages, and than the replacement of the hydrogen of marsh-gas by chlorine at four successive stages.

But the formation of ether from water by Williamson's process, affords a still closer resemblance to Hofmann's formation of triethylamine from ammonia. Water $\text{H}_2\text{O}''$, when acted upon by potassium, yields hydrate of potassium, or potassic water KHO'' . Hydrate of potassium acted upon by iodide of ethyl yields alcohol, or ethylated water EtHO'' . Alcohol acted upon by potassium yields potassium-alcohol, or ethylated potassic water EtKO'' . Potassium-alcohol acted on by iodide of methyl yields the first mixed ether, or methylated ethyl-water EtMeO'' ; and when acted upon by iodide of ethyl yields common ether, or ethylated ethyl-water $\text{Et}_2\text{O}''$. None of the intermediate bodies can be represented save with 16 parts of oxygen, and hence the terminal bodies being members of the same series, must be also represented with 16 parts of oxygen. The intimate relations and analogies of ethylated methyl-water EtMeO'' and diethylated water $\text{Et}_2\text{O}''$, as regards modes of formation, modes of decomposition, boiling point, vapour-density, &c., will not allow the molecule of the mixed ether to be represented with 16 parts, and that of the common ether with only 8 parts of oxygen; any more than the relations and analogies of ethyl-methyl-phenylamine $\text{EtMePhN}'''$ and triethylamine $\text{Et}_3\text{N}'''$ will allow the molecule of the former compound to be represented with 14 parts, and that of the latter with only 4·7 parts of nitrogen.

With regard to chlorhydric acid, there is no intermediate stage in the replacement of its hydrogen; the substitution taking place at once or not at all. We have, for instance, chloride of hydrogen HCl , chloride of ethyl EtCl , and chloride of sodium NaCl , but no intermediate

bodies ; whence the representation of chlorhydric acid by the simple monhydric model, thus :—



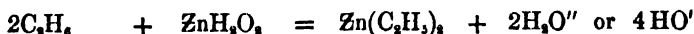
In addition to the above-mentioned chemical parallel, there is one very remarkable physical relation subsisting between marsh-gas, ammonia, water, and chlorhydric acid, namely, that all four molecules occupy the same bulk. Two cubic feet, for instance, of chlorhydric acid-gas HCl, yield 1 cubic foot of hydrogen and 1 cubic foot of chlorine. Two cubic feet of steam H_2O , yield 2 cubic feet of hydrogen and 1 cubic foot of oxygen. Two cubic feet of ammonia H_2N , yield 3 cubic feet of hydrogen and 1 cubic foot of nitrogen ; while two cubic feet of marsh-gas HC , yield 4 cubic feet of hydrogen and a quantity of carbon of which the gaseous volume has not been and cannot be ascertained. With equal volumes of all four gases, the marsh-gas contains four times as much, the ammonia three times as much, and the steam twice as much hydrogen as the chlorhydric acid.

Many other arguments may be adduced to show that the molecular weight of water is 18, corresponding to the formula H_2O'' , and not 9, corresponding to the formula HO' . Thus water is concerned as the agent or product of some chemical reaction more frequently than any other body with which chemists are acquainted ; but in no well-defined reaction do we ever find the reacting or resulting water expressible by 9 parts, or three times 9 parts, or five times 9 parts, &c. but in every case by 18 parts, or some multiple of 18 parts, as instanced in the following examples, which might be increased indefinitely :—

Formation of Nitro-benzol.



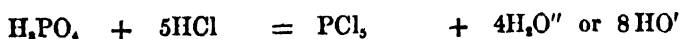
Decomposition of Zinc-ethyl.



Formation of Hydro-benzamide.



Decomposition of Pentachloride of Phosphorus.



Again, in the great majority of direct compounds which water forms with other bodies, the combining water necessarily constitutes 18 parts, or some multiple of 18 parts; and when two or more bodies differ from one another by the quantities of water they respectively contain, the differential quantity always amounts to 18 parts or some multiple of 18 parts, thus :—

		$C_{10}H_{16}$	Turpentine.
$C_{10}H_{16} \cdot H_2O$	or	$C_{10}H_{18}O$	Monhydrate.
$C_{10}H_{16} \cdot 2H_2O$	or	$C_{10}H_{20}O_2$	Dihydrate.
$C_{10}H_{16} \cdot 3H_2O$	or	$C_{10}H_{22}O_3$	Trihydrate.
		$CNII$	Prussic Acid.
$CNH \cdot H_2O$	or	$CNII_3O$	Formamide.
$CNH \cdot 2H_2O$	or	CNH_5O_2	Formate of Ammonia.

Several chemists, during what may be termed the transitional stage of their views, were accustomed to represent the molecule of water by the formula $H_2O'_2$, in which O' stands for 8 parts of oxygen, instead of by the formula H_2O'' , in which O'' stands for 16 parts by weight of oxygen. But the use of this formula really showed an imperfect appreciation of the grounds upon which the abandonment of the simpler formula HO' was based; for if the molecule of water is to be represented with two proportions of hydrogen, because its hydrogen is divisible into two parts, *invertendo* it must be represented with but one proportion of oxygen, because its oxygen is indivisible. Now that the oxygen of water is indivisible is acknowledged alike by those who use the formula H_2O'' and those who use, or rather used, the formula $II_2O'_2$; and such being the case, the representation of the oxygen of water by the expression O'_2 is quite unwarrantable, for the conception of two inseparable proportions of oxygen having each the value 8, amounts after all to that of a single indivisible proportion of oxygen having the value 16.

That the oxygen not only of water but of all other well-defined oxygenated molecules always amounts to 16 parts, or some multiple of 16 parts, is abundantly evident. For the composition alone of the great majority of oxygenated bodies, prevents them from being represented save with 16 or some multiple of 16 parts of oxygen, just as the composition of chloroform prevents its being represented save with 12 parts of carbon; and as the composition of ethylamine prevents its being represented save with 14 parts of nitrogen. With regard to the remaining oxygenated bodies, their mere composition would allow them to be represented with only 8 or some odd multiple of 8 parts of oxygen, but their several relations to other bodies belonging to the more numerous class prevents them from being represented with so

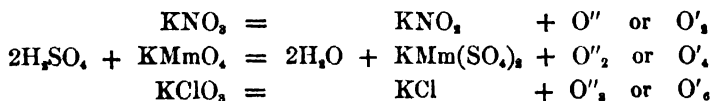
small a quantity; just as the relation of triethylamine to ethylamine and diethylamine prevents it from being represented with less than 14 parts of nitrogen, though from its mere composition it might be represented with only 4·7 parts of nitrogen.

From this it follows, that when two or more bodies differ in composition from one another by the proportions of oxygen they respectively contain, that difference always amounts to 16 parts, or some multiple of 16 parts, as shown in the following series:—

H Cl	H Cl	Chlorhydric acid.
H Cl O',	H Cl O''	Hypochlorous acid.
H Cl O',	H Cl O'',	Chlorous acid.
H Cl O',	H Cl O'',	Chloric acid.
H Cl O',	H Cl O'',	Perchloric acid.
C ₄ KH ₃ O',	C ₄ KH ₃ O'',	Acid succinate.
C ₄ KH ₃ O' ₁₀	C ₄ KH ₃ O'',	Acid malate.
C ₄ KH ₃ O' ₁₈	C ₄ KH ₃ O'',	Acid tartrate.

Throughout there is no difference of 8 parts of oxygen or of any odd multiple of 8 parts of oxygen, between the successive compounds.

It follows also that whenever oxygen, with or without some other element, is added to or taken from a compound, the oxygen so added or eliminated must always be represented by 16, or some multiple of 16 parts, as instanced in the following decompositions, representing the liberation of oxygen from nitrate, permanganate, and chlorate of potassium respectively:—



Let the decompositions be expressed how they may, the oxygen displaced cannot appear as 8 parts, or any odd multiple of 8 parts; and so in hundreds of other instances.

The representation of the molecule of water by the formula H₂O was proposed by Gay-Lussac, advocated by Berzelius, and employed by Davy more than fifty years ago. Its revival is due to Gerhardt, who in conjunction with Laurent made it the basis of a consistent system of chemistry, since brought to a higher state of development by the labours of many distinguished chemists, including more especially

Professor Williamson, who first introduced it into this country and supported it by a series of most masterly researches.

In accordance with this system, four well-defined classes of primary hydrides are recognized, the leading member of each of which constitutes the type or model to which an infinite variety of compounds are referable as regards both their composition and behaviour ; thus :—

Monhydrides.	Dihydrides.	Trihydrides.	Tetrahydrides.
H F	H ₂ O	H ₃ N	H ₄ C
H Cl	H ₂ S	H ₃ P	H ₄ Si
H Br	H ₂ Se	H ₃ As	
H I	H ₂ T	H ₃ Sb	

Formulated as above, all these hydrides occupy the same volume, and their respective vapour-densities compared with that of hydrogen as unity are the halves of their atomic weights.

[W. O.]

Friday, May 22, 1863.

The Rev. JOHN BARLOW, M.A. F.R.S. Vice-President, in the Chair.

PROFESSOR ROSCOE, B.A. F.C.S.

On the Direct Measurement of the Sun's Chemical Action.

THE life of the animal may be described chemically as a process of oxidation : the tissues of his body are continually undergoing combustion ; he is constantly breathing out carbonic acid gas, and thus deteriorating the ocean of air at the bottom of which he lives and moves ; so that, were not a counteracting influence at work, he would, during each moment of his existence, be working his own destruction. This counteracting influence is exerted by vegetables, whose life is chemically characterised by a change opposite to that of the animal, that, namely, of deoxidation or reduction. Animals take up oxygen and give off carbonic acid ; plants reverse the process, they take up carbonic acid and give off oxygen ; and thus the balance of atmospheric life is kept always true.

The animal derives its power from the forces locked up in the vegetable organisms which constitute its food, and of which it builds up its tissues. When they are destroyed by the action of the atmospheric oxygen, these forces become evident either in motion of the masses, constituting mechanical action, or in the motions of the particles constituting heat, or other manifestations of energy. The animal cannot create force ; he can only direct its application : he cannot move a muscle without a certain given quantity of force being changed, without a certain portion of his tissues undergoing oxidation, an amount which is regulated by the grand principle of the conservation of force—so that the total energy which the animal exhibits is regulated by the same laws which apply to the work of the steam or electro-magnetic engine. Every pound of carbon burnt to carbonic acid in the animal body evolves heat enough to raise the temperature of 8080 lbs. of water 1° centigrade, or can produce a mechanical effect sufficient to raise 2784 tons one foot high.

The source of the power of the animal is evident ; it lives upon the force which has been accumulated by the plant. The animal world cannot continually withdraw energy from the plant, unless the latter receives as continual a supply. The source of this energy is the sun ; the plant sucks up or absorbs the rapidly vibrating solar radiations and stores them up to be given out again in the various forms of energy when the vegetable tissue is destroyed by oxidation.

It is only in the presence of the sunlight that the true function of plant life can be exercised. It is the sunlight which, acting on the green colouring matter of leaves, decomposes the carbonic acid of the air into its constituent elements, enabling the plant thus to assimilate the carbon and to turn the free oxygen back into the air.

Only those of the solar rays which vibrate the most rapidly are able thus to tear the particles of carbon and oxygen asunder, or to effect chemical change; and these most refrangible or violet rays have, therefore, been called the chemical rays—not that there is any difference in kind between these and the other solar radiations; they all differ only in wave-length and in intensity of vibration.

These blue rays then, falling on the green portions of plants, are absorbed to do work; their rapid vibrations are used up to set free the carbon and oxygen, and the heat equivalent to these absorbed vibrations is again given off when the carbon thus produced is burnt.

The speaker illustrated the chemical activity of the blue rays, and the inactivity of the red rays, by showing that a bulb filled with chlorine and hydrogen explodes when exposed to an intense blue light, but is unaffected upon by an equally bright red light.

The measurement of the amount of these chemically active rays falling at a given time upon a given spot, must be of the highest meteorological interest, as their variation forms a most important element in the changing plant- and animal-producing power of a country.

Three years ago (March 2, 1860) the speaker brought forward the results of a series of experiments undertaken by Professor Bunsen and himself, for the purpose of obtaining a means of accurately measuring the chemical action of sunlight.

The measure consisted in the quantity of hydrochloric acid formed by the action of light upon a mixture of equal volumes of chlorine and hydrogen gases. The authors succeeded in arranging a most accurate and reliable chemical photometer, by help of which the laws regulating the chemical action of light were investigated, and the distribution of direct and diffuse sunlight upon the earth's surface determined when the sky is unclouded.* The delicate nature of the instrument, which the sensitive substance rendered necessary, precludes the general use of this chlorine and hydrogen photometer, and the method is likewise inapplicable when we wish to measure the total effect produced by the varying cloud and sunshine of our changing climate.

The object of the present research, carried on by Professor Bunsen and the speaker, has been to invent an easy and reliable mode of measuring the daily variation in the sun's chemical intensity which shall be applicable to regular meteorological registration.

Thermometric observations, giving the mean monthly or yearly temperature of a country, by no means yield all the data required for the estimation of the true climatology of the place, or of its plant- and

* Phil. Trans. for 1857, pp. 355, 381, 601; for 1859, p. 879.

animal-producing capabilities. For this purpose we require to have not only the amount of solar heat directly or indirectly reaching the spot; but likewise the amount of chemically active solar light which falls there. This is strikingly seen when we compare the mean annual temperature of Thorshavn (Faroe Islands) with that of Carlisle.

				Temp. Fah.	Diff.
Thorshavn	N. lat. 62° 2'	W. long. 6° 46'	. .	45°·6	} 1°·3.
Carlisle	— 54° 54'	— 2° 58'	. .	46°·9	

In these two situations the mean annual temperature is nearly equal, but the quantity of sunlight falling upon these two places differs most widely, and we have a corresponding difference in the true climatological relations. Owing to the constant moisture and cloudy state of the atmosphere, which the sun's chemical rays cannot penetrate, the flora of the Faroes and Shetlands is of the most limited description, only the most hardy variety of shrubs and no trees flourishing; whilst at Carlisle we have the most luxuriant vegetation accompanying a more sunny sky. Thus, too, the mean summer temperature of Reykjavik in Iceland is only 3° 8 Fah. below that of Edinburgh, whilst the difference between the mean summer temperature of Edinburgh and London is 5°·4 Fah. Yet in Iceland no tree grows; whilst between London and Edinburgh, we notice no marked difference as regards the development of vegetable life. Hence we see that places upon the same isothermal do not necessarily possess a truly corresponding climate; this can only be attained when, amongst other conditions, the places are situated upon the curve of equal mean chemical intensity.

Although many fruitless attempts have been made to construct photometers by a comparison of the blackness produced by sunlight upon photographic paper, it was found that in this way the desired end could be attained by attention to certain essential conditions.

For this purpose it was necessary to construct an apparatus in which photographic sensitized paper could be exposed to the sunlight for definite times measured by small fractions of a second. This instrument consists essentially of a pendulum vibrating about $\frac{1}{4}$ -seconds, by whose oscillation a sheet of darkened mica is withdrawn from, and brought back over, a horizontal strip of paper prepared with chloride of silver, and fixed in a constant position relative to the pendulum and sheet of mica. The time during which each point in the length of the strip is exposed is different, and the time of exposure for each point can be calculated when the length and position of the strip, and the duration and amplitude of the pendulum's vibration are given.

The strip of sensitive paper presents, after exposure, a gradual diminution of shade from dark to light, and for each shade the time of exposure is known. In order that such a graduated strip may serve as a means of measuring the chemical action of light, we require:—

1. To know the relation existing between the several tints and the intensity of the light necessary to produce such tints.

2. To construct sensitive paper which shall always possess the

same degree of sensitiveness, and can easily be prepared when required.

It was found, by a long series of experiments, that it was possible, by adhering strictly to a certain method of manipulation, to prepare standard papers which, when made, possess a constant degree of sensitiveness; so that if the same light falls upon them, the papers are always coloured to the same tint.

Experiment likewise showed that the tint attained by such a paper was constant when the *quantity* of light falling upon it also remained constant; so that light of the intensity 50 falling upon the paper for the time 1, produced the same blackening effect as light of the intensity 1 falling upon it for the time 50.

Knowing this law, which regulates the degree of shade of the paper, and having a surface of a perfectly constant degree of sensitiveness, it is easy to obtain absolute measurements of the chemical action of light. For this purpose an arbitrary unit of measurement is chosen, by making a standard tint or paint which can be easily and exactly reproduced at any time.

The quantity of light which shall, in a second or the unit of time, produce a blackening effect on the standard photographic paper equal to that of the standard tint, is said to have the chemical intensity 1. If the time needed to produce this same tint is found by experiment with the pendulum-photometer to be 2 seconds, then the chemical intensity is one-half, and so on.

All that is needed, in order to obtain accurate measurements of the chemical action of diffuse daylight or sunlight, is to be able to find the time necessary to effect a blackening of the normal paper equal in shade to the standard tint. This is done by means of the graduated strip made in the pendulum-photometer.

For the purpose of accurately comparing these tints, the ordinary daylight or even lamplight cannot be used, as a change would thereby be produced on the sensitive paper. A light which does not act chemically must be used; such a light is the monochromatic soda-flame. The light from this flame possesses another advantage, namely, that the unavoidable differences of colour are not seen; variation in *shade* alone being perceptible. The speaker exhibited the accuracy of this method of observing coincidence in shade by means of a large model of the instrument.

By help of this soda-flame the coincidence of shade of the graduated strip with the standard tint can be read off with the greatest precision. This fact, as well as the possibility of preparing a constant sensitive paper, is seen by reference to the following tables, extracted from the detailed paper printed in the Philosophical Transactions for 1863.

Papers variously prepared were exposed for the same time to the same light.

Each reading is the mean of several observations; identity in the numbers shows identity in the shade, and, therefore, the constant sensitiveness of the papers.

The standard paper is prepared by soaking photographic paper in a solution of common salt of given strength (3 to 100), and then allowing it to lie upon the surface of a silver solution (12 NO_3Ag to 100 of water). When the strength of salt solution varies, the sensitiveness of the paper alters very rapidly. Variation in the strength of the silver-bath produces, on the contrary, but little change in the sensitiveness of the paper. Different qualities of paper and alterations of atmospheric moisture and temperature do not affect the sensitiveness of the paper.

1. *Effect of altering the Strength of the Silver-bath. Paper salted in a Solution containing 3 parts Chloride of Sodium to 100 of Water.*

Ag NO_3 to 100 of water.	READINGS.		
	Observer A.		Observer B.
12	128.6	..	129.7
10	128.7	..	127.0
8	128.7	..	128.0
6	129.7	..	130.0

2. *Effect of altering the Strength of the Salt Solution.*

Na Cl to 100 of water.	READINGS		
	Observer A		Observer B
1	62.6	..	60.4
2	95.7	..	94.6
3	132.6	..	129.6
4	167.0	.	168.0

3. *Experiment showing the constant Sensitiveness of the Standard Paper.*

Paper.	Na Cl to 100 of water	Intensity No 1		Intensity No 2.	
		Observer A	Observer B	Observer A.	Observer B.
Upper part of sheet 2	2.950	70.2	70.0	101.3	101.5
Lower part of sheet 2	3.026	70.6	69.3	101.5	101.7
Middle of sheet 1 .	3.026	70.0	69.5	100.9	100.9
Middle of sheet 3 .	3.000	70.0	70.4	101.0	100.0

All these papers were silvered in a solution containing 12 parts of nitrate of silver to 100 of water.

In order to measure the chemical intensity of the daylight at any time, all that is needed is to expose a strip of standard paper in the pendulum-photometer for a given number of vibrations, and then to

find upon the strip, thus exposed, the point at which a shade equal to the standard tint has been produced. Reference to a table gives the time of exposure necessary to produce this tint, and the reciprocal of this time represents the intensity of the acting light. If the time necessary were 3 seconds, the chemical intensity would be $\frac{1}{3}$; if the time were $\frac{1}{2}$ second, the intensity would be 2. In this way curves of daily chemical intensity were exhibited, which show the variation caused by clouds, or by the changing altitude of the sun. These curves show maxima and minima exactly corresponding to the appearance and disappearance of the sun behind a cloud. The difference between the sun's chemical intensity in summer and winter is thus also clearly depicted.

Based upon the principles of the pendulum-photometer, a much simpler method of making these measurements has been arranged, as follows. A graduated strip made in the pendulum-photometer is fixed in hyposulphite of soda, and pasted upon a board furnished with a scale. The shades of certain points on this fixed strip are compared with the shades on given points upon a graduated strip prepared in the usual way, and not fixed in hyposulphite. The fixed strip is thus calibrated in terms of the unit of measurement, and it may then be used as a means of measuring the chemical action of light. Small pieces of the standard paper are then exposed for a given time to the light which it is desired to measure, until the shade approaches that of a part of the fixed strip. The point of exact coincidence is then read off by the soda flame as usual. In this way a piece of standard paper of 1 square-inch of area will serve for 20 separate determinations, and the whole arrangement for exposure may be carried in the pocket. The curve of the chemical intensity of day and sunlight in Manchester, on May 15th, 1863, made with this small instrument, fully bears out the accuracy and ease with which these measurements can be made, and the results of his experiments induced the speaker to express a hope that before long these instruments may be introduced into meteorological observatories.

The determination of the chemical brightness of the various portions of the sun's disc is an interesting application of this new method of photometric measurements.

By help of a camera placed on a 3-inch refractor, the speaker allowed the image of the sun—of about 4 inches in diameter—to fall upon the standard paper. The sun-picture thus obtained presents interesting features; in the first place, the chemical intensity of the central portions are 3–5 times as great as that of the portions on the limb. A difference of this kind, in the case of the luminous and calorific rays, has already been observed by astronomers, and it is doubtless caused by the absorption effected by the solar atmosphere.

The following results were obtained by measuring the chemical brightness at various points on the sun's disc, on May 9th, 1863; from these numbers it will be seen that the luminous intensity varies very irregularly.

CHEMICAL BRIGHTNESS OF SUN'S DISC.

1. At Centre of Sun's Disc.	2. 15° from Edge of Sun's Disc.			3. At the Edge of Sun's Disc.		
	N. Pole.	Equator.	S. Pole.	N. Pole.	Equator.	S. Pole.
No. 1. .. 100·0	38·8	. 48·4	. 58·1	18·7	. 30·2	. 28·2
No. 2. .. 100·0	52·8	. —	. 56·6	30·5	. —	. 41·0

Bright patches of considerable area were seen on the picture; these patches, which were not caused by irregularity in the paper or in the lenses, are probably owing to the presence of clouds in the luminous atmosphere of the sun, and they may probably have some intimate connection with the well-known phenomena of the red prominences seen during the solar eclipse.

The speaker concluded by stating that he hoped, with the assistance of his friend Mr. Baxendell, to make a series of regular observations of the variation of the chemical intensities of many points on the sun's surface.

[H. E. R.]

Friday, June 12, 1863.

REV. JOHN BARLOW, M.A. F.R.S. Vice-President, in the Chair.

JOHN TYNDALL, Esq. F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION.

An Account of some Researches on Radiant Heat.

IN his former researches on the radiation and absorption of heat by gaseous matter, the speaker compared different gases and vapours at a common thickness with each other; one part of his present object was to compare different thicknesses of the same gaseous body with each other as to their action upon radiant heat. A few years ago he would be deemed a bold man who would attempt to measure the action of an inch, or indeed of many feet of a gas, on radiant heat; but the present experiments commence with plates of gas only 0.01 of an inch in thickness, and extend to thicknesses of 49.4 inches. Thus, the greatest thickness is to the least nearly in the ratio of 1 to 5000. The apparatus employed for the smaller thicknesses was a hollow cylinder, one end of which was closed by a plate of rock-salt. Into this fitted a second cylinder, with its end also closed by a plate of the salt. One cylinder moved within the other like a piston, and, by this means the two plates of salt could be brought into flat contact with each other, or could be separated to any required distance. The distance between the plates was measured by a *vernier*. The cylinder was placed horizontal, being suitably connected with a source of heat. This latter consisted of a plate of copper, against which a steady sheet of flame was caused to play.

The absorption of radiant heat by carbonic oxide, carbonic acid, nitrous oxide, and olefiant gas was determined with this apparatus, and such differences as might be anticipated from former researches were found. Olefiant gas maintained its great superiority over the other gases at all thicknesses. A layer of this gas, not more than 0.01 of an inch in thickness, intercepted about 1 per cent. of the total radiation; and the delicacy of the apparatus may be inferred from the fact that this absorption—great, relative to the thickness of the layer of gas, but small absolutely—corresponded to a deflection of 11 degrees of the galvanometer. (It would be certainly possible to measure the action of a layer of this gas of less thickness than the paper on which these words are printed.) A layer of olefiant gas, 2 inches in thickness, intercepts nearly 30 per cent. of the entire radiation. The influence of a diathermic envelope surrounding a planet may be strikingly illus-

trated by reference to this gas. A shell of olefant gas, 2 inches thick, surrounding the earth, would offer no appreciable hindrance to the solar rays in their earthward course; but it would intercept, and in great part return, 30 per cent. of the terrestrial radiation: under such a canopy the surface of the earth would probably be raised to a stifling temperature. A layer of the gas, 3-10ths of an inch thick, intercepts 11·5 per cent. of the whole radiation. Such a layer, if diffused through a stratum of air 10 feet thick, would be far more attenuated than the aqueous vapour actually diffused through the air; still it would produce an absorption greater than that which the speaker had assigned to the atmospheric vapour within 10 feet of the earth's surface. In the presence of such facts, the arguments which we might be disposed to base on the smallness of the quantity of atmospheric vapour are entirely devoid of weight.

In measuring the action of larger thicknesses of gas, the following method was pursued:—A brass cylinder, 49·4 inches in length, had its two ends stopped with plates of rock salt, and a suitable source of heat placed at one end; the rays from this source passed through the tube, and were received by a thermo-electric pile placed at its opposite end; this radiation was exactly neutralized by the heat emitted from a cube of boiling water and incident on the opposite face of the pile. The interception of any portion of the heat emanating from the source by a gas or vapour introduced into the tube destroyed the equilibrium previously existing, and the amount intercepted was declared by the galvanometer. The thickness traversed by the calorific rays was varied in the following way:—The tube was divided into two distinct compartments by the introduction of a third plate of rock salt. Let us agree to call the compartment most distant from the pile the *first chamber*, and that adjacent to the pile the *second chamber*. The experiments began with the first chamber short and the second chamber long, and ended with the first chamber long and the second chamber short. The alteration consisted solely in the shifting of the intermediate plate of salt, which lengthened the first chamber and diminished the second one by the same quantity; the sum of the lengths of both chambers being the constant quantity, 49·4 inches.

The absorption effected in the first chamber acting alone was first determined; then the absorption effected in the second chamber acting alone; and, finally, the absorption effected when both the chambers were occupied by the gas or vapour. This arrangement enabled the speaker to check his experiments, and also to examine the effect of the *sifting* which occurred in the first chamber on the absorption of the second one. The thermal coloration of the various gases was rendered strikingly manifest by these experiments. For the vast majority of the rays, for example, carbonic oxide and carbonic acid are transparent. Placing a stratum of carbonic oxide, 8 inches in length, in front of a column of the same gas, 41·4 inches long, these 8 inches intercepted 6 per cent. of the whole radiation; placed *behind* a column, 41·4 inches long, the absorption of the same 8 inches was

sensibly *nil*. So also with carbonic acid; 8 inches in front absorbed $6\frac{1}{2}$ per cent., while placed behind the effect was almost zero. Similar remarks apply to the other gases, the reason manifestly being that when the 8-inch stratum is in front, it stops the main portion of the rays which give it its thermal colour, while, when it is placed behind, these same rays have been almost wholly withdrawn, and to the remaining 94 per cent., or thereabouts, of the radiation the gases are sensibly transparent.

An extension of this reasoning enables us at once to conclude, that the sum of the absorptions of the two chambers taken separately must always be greater than the absorption effected by a single column of the gas of a length equal to the sum of the two chambers. This conclusion is illustrated in a striking manner by the experiments; and it is further found that when the mean of the sums of the absorptions is divided by the absorption of the sum, the quotient is sensibly the same for all gases. It may also be inferred from considerations similar to the foregoing, that the sum of the absorptions must diminish, and approximate to the absorption of the sum, as the two chambers become more unequal in length, and that the sum of the absorptions of the two chambers is a maximum, when the medial rock-salt plate divides the long tube into two equal compartments.

In these days a special interest attaches itself to the radiation of any gas through itself or through any other gas having the same period of vibration. The speaker referred to the results of an elaborate series of experiments on this interesting question. The experimental tube, 49.4 inches long, was divided into two compartments by a partition of rock-salt. All external sources of heat were abolished, and the pile, furnished with its conical reflector, stood at the end of the tube. The compartment nearest the pile contained the gas which was to act as absorber, while that most distant from the pile held the gas which was to act as radiator. It is known that the destruction of the motion of a sensible mass of matter is always accompanied by the evolution of heat. A weight falling to the earth, and a ball striking a target, are heated on collision. The same is true for atoms, and in the present experiments the gas in the radiating chamber was heated by the collision of its own particles against the inner surface of the tube when they rushed in to fill the vacuum. The radiation was, in fact, what the speaker had named "dynamic radiation." The lengths of the two chambers were varied, the radiating column being lengthened and the absorbing one shortened at one and the same time; the sum of both was always the constant length 49.4 inches.

The experiments with the vapours were thus executed. Both the chambers into which the tube was divided were, in the first place, occupied by the vapour to be examined; the usual pressure being 1.60th of an atmosphere. The entrance of the vapour was so slow, and its quantity so small, that the radiation due to the warming of the vapour by its own collision was insensible. The needle being at zero, dry air was allowed to enter the chamber most distant from the pile. This

air became heated dynamically, communicated its heat to the vapour, and the latter immediately discharged the heat thus communicated to it against the pile. It is quite evident, that not only does this case resemble, but that it is actually of the same mechanical character as that in which a vibrating tuning-fork is brought into contact with a surface of some extent. The fork, which before was inaudible, becomes at once a copious source of sound. What the sounding-board is to the fork, the compound molecule is to the elementary atom. The tuning-fork vibrating alone is in the condition of the atom radiating alone, the sound of the one and the heat of the other being alike insensible. But in association with sulphuric or acetic ether-vapour the elementary atom is in the condition of the tuning-fork applied to its sound-board, communicating through the molecule motion to the luminiferous ether, as the fork through the board communicates its motion to the air.

The experiments demonstrate the great opacity of a gas to radiations from the same gas. They also show in a very striking manner the influence of attenuation in the case of vapour. The individual molecules of a vapour may be powerful absorbers and radiators, but in thin strata they constitute an open sieve through which a large quantity of radiant heat may pass. In such thin strata, therefore, the vapours, as used in our experiments, were generally found far less energetic than the gases, while in thick strata the same vapours showed an energy greatly superior to the same gases. The gases, it will be remembered, were always employed at a pressure of one atmosphere.

A few striking experiments were referred to in illustration of the influence of a paper lining, or a coat of varnish or lampblack, within the experimental tube. In dynamic radiation it is not possible to do entirely away with the action of the interior surface of the tube itself. When the tube is of brass and well polished within, the entrance of the air produces a deflection of 7.5 degrees, this being due to the emission from the warmed surface of the tube. A lining of paper two feet long raises the radiation sufficiently to drive the needle through an arc of 80 degrees, while a ring of paper 1½ inches long placed within the tube radiates sufficient to urge the needle through an arc of 56 degrees.

The speaker finally examined the diathermancy of the liquids from which his vapours were derived, and the result leaves no shadow of a doubt upon the mind, that both absorption and radiation are molecular phenomena, irrespective of the state of aggregation. If any vapour is a strong absorber and radiator, the liquid whence it comes is also a strong absorber and radiator. The molecule carries its power, or want of power, through all its states of aggregation. The order of absorption in liquids and vapours is precisely the same; and the speaker looked forward with hope to the application of these results to other portions of the domain of thermotics.

[J. T.]

Friday, January 22, 1864.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. Vice-President,
in the Chair.

W. R. GROVE, Esq. Q.C. F.R.S. M.R.I.

On Boiling Water.

A PAPER by M. Donny ('Mémoires de l'Académie Royale de Bruxelles,' 1843) makes known the fact that in proportion as water is deprived of air, the character of its ebullition changes, becoming more and more abrupt, and boiling like sulphuric acid with *soubresauts*, and that between each burst of vapour the water reaches a temperature above its boiling point. To effect this, it is necessary that the water be boiled in a tube with a narrow orifice, through which the vapour issues; if it be boiled in an open vessel, it continually re-absorbs air and boils in the ordinary way.

In my experiments on the decomposition of water by heat, I found that with the oxy-hydrogen gas given off from ignited platinum plunged into water, there was always a greater or less quantity of nitrogen mixed; this I could never entirely get rid of, and I was thus led into a more careful examination of the phenomenon of boiling water, and set before myself this problem—what will be the effect of heat on water perfectly deprived of air or gas?

Two copper wires were placed parallel to each other through the neck of a Florence flask, so as nearly to touch the bottom, joining the lower ends of these was a fine platinum wire, about $1\frac{1}{2}$ inch long, and bent horizontally into a curve. Distilled water, which had been well boiled and cooled under the receiver of an air-pump, was poured into this flask so as to fill about one-fourth of its capacity. It was then placed under the receiver of an air-pump, and one of the copper wires brought in contact with a metallic plate covering the receiver, the other bent backwards over the neck of the flask, and its end made to rest on the pump plate. By this means, when the terminal wires from a voltaic battery were made to touch, the one the upper and the other the lower plate, the platinum wire would be heated, and the boiling continued indefinitely in the vacuum of a very excellent air-pump. The effect was very curious; the water did not boil in the ordinary manner, but at intervals a burst of vapour took place, dashing the water against the sides of the flask, some escaping into the receiver. (There was a projection at the central orifice of the pump-plate to prevent this overflow getting into the exhausting tube.)

After each sudden burst of vapour, the water became perfectly

tranquil, without a symptom of ebullition until the next burst took place. These sudden bursts occurred at measured intervals, so nearly equal in time, that, had it not been for the escape from the flask, at each burst, of a certain portion of water, the apparatus might have served as a timepiece.

This experiment, though instructive, did not definitely answer the question I had proposed, as I could not of course ascertain whether there was some minute residuum of gas which would form the nucleus for each ebullition; and I proceeded with others. A tube of glass, 5 feet long and $\frac{1}{16}$ ths inch internal diameter, was bent into a V shape; into one end a loop of platinum wire was hermetically sealed with great care, and the portion of it in the interior of the tube was platinized. When the tube had been well washed, distilled water, which had been purged of air as before, was poured into it to the depth of 8 inches, and the rest of the tube filled with olive oil; when the V was inverted, the open end of the tube was placed in a vessel of olive oil, so that there would be 8 inches of water resting on the platinum wire, separated from the external air by a column of 4 feet 4 inches of oil. The projecting extremities of the platinum wire were now connected with the terminals of a voltaic battery and the water heated; some air was freed and ascended to the level of the tube—this was made to escape by carefully inverting the tube so as not to let the oil mix with the water—and the experiment continued. After a certain time the boiling assumed a uniform character, not by such sudden bursts as in the Florence flask experiment, but with larger and more distinct bursts of ebullition than in its first boiling.

The object of platinizing the wire was to present more points for the ebullition, and to prevent *soubresauts* as much as possible.

The experiment was continued for many hours, and in some repetitions of it for days. After the boiling had assumed a uniform character, the progress of the vapour was carefully watched, and as each burst of vapour condensed in the oil, which was kept cool, it left a minute head of gas, which ascended through the oil to the bend of the tube: a bubble was formed here which did not seem at all absorbed by the oil. This was analyzed by a eudiometer, which I will presently describe, and proved to be nitrogen. The beads of gas, when viewed through a lens and micrometer scale at the same height in the tube, appeared as nearly as may be of the same size. No bubble of vapour was condensed completely, or without leaving this residual bubble. The experiment was frequently repeated, and continued until the water was so nearly boiled away, that the oil, when disturbed by the boiling, nearly touched the platinum wire; here it was necessarily stopped.

To avoid any question about the boiling being by electrical means, similar experiments were made with a tube, without a platinum wire, closed at its extremity, and the boiling was produced by a spirit-lamp. The effects were the same, but the experiment was more difficult and imperfect, as the bursts of vapour were more sudden, and the duration of the intervals more irregular.

The beads of gas were extremely minute, just visible to the naked eye, but were made visible to the audience by means of the electric lamp.

In these experiments there was no pure boiling of water, *i. e.* no rupture of cohesion of the molecules of water itself, but the water was boiled, to use M. Donny's expression, by evaporation against a surface of gas.

It is hardly conceivable that air could penetrate through such a column of oil, the more so as the oil did not perceptibly absorb the nitrogen freed by the boiling water and resting in the bend of the tube; but to meet this conjectural difficulty, the following experiment was made. A tube, 1 foot long and $\frac{1}{10}$ ths inch internal diameter, bent into a slight angle, had a bulb of $\frac{1}{2}$ -inch diameter blown on it at the angle; this angle was about 3 inches from one end and 9 from the other; a loop of platinum wire was sealed into the shorter leg, and the whole tube and bulb filled with and immersed into mercury; water, distilled and purged of air as before, was allowed to fill the short leg, and by carefully adjusting the inclination, the water could be boiled so as to allow bubbles to ascend into the bulb and displace the mercury. The effect was the same as with the oil experiment, no ebullition without leaving a bead of gas; the gas collected in the bulb, and was cut off by what may be termed a valve of mercury, from the boiling water, then allowed to escape, and so on; the experiment was continued for many days, and the bubbles analyzed from time to time; they proved, as before, to be nitrogen; and, as before, continued indefinitely.

A similar experiment was made without the platinum wire, and though, from the greater difficulties, the experiment was not so satisfactory, the result was the same.

As the mercury of the common barometer will keep air out of its vacuum for years, if not for centuries, there could be no absorption here from the external atmosphere, and I think I am fairly entitled to conclude from the above experiments—which I believe went far beyond any that have been recorded—that no one has yet seen the phenomenon of pure water boiling,—*i. e.* of the disruption of the liquid particles of the oxy-hydrogen compound so as to produce vapour which will, when condensed, become water, leaving no permanent gas. Possibly, in my experiment of the decomposition of water by ignited platinum, it may be that the sudden application of intense heat, and in some quantity, so forces asunder the molecules that, not having sufficient nitrogen dissolved to supply them with a nucleus for evaporation, the integral molecules are severed, and decomposition takes place. If this be so, and it seems to me by no means a far-fetched theory, there is probably no such thing as boiling, properly so called, and the effect of heat on liquids in which there is no dissolved gas may be to decompose them.

Considerations such as these led me to try the effect of boiling on an elementary liquid, and bromine occurred as the most promising one to work upon; as bromine could not be boiled in contact with water, oil, or mercury, the following plan was ultimately devised. A tube, 4 feet long and $\frac{1}{10}$ ths inch diameter, had a platinum loop sealed into

one closed extremity ; bromine was poured into the tube to the height of 4 inches ; the open end of the tube was then drawn out to a fine point by the blow-pipe, leaving a small orifice ; the bromine was then heated by a spirit-lamp ; and when all the air was expelled, and a jet of bromine vapour issued from the point of the tube, it was sealed by the blow-pipe. There was then, when the bromine vapour had condensed, a vacuum in the tube above the bromine. The platinum loop was now heated by a voltaic battery, and the bromine boiled : this was continued for some time, care being taken that the boiling should not be too violent. At the end of a certain period—from half-an-hour to an hour—the platinum loop gave way, being corroded by the bromine ; the quantity of this had slightly decreased. On breaking off, under water, the point of the tube, the water mounted and showed a notable quantity of permanent gas, which on analysis proved to be pure oxygen. As much as a quarter of a cubic inch was collected at one experiment. The platinum wire, which had severed at the middle, was covered with a slight black crust, which, suspecting to be carbon, I ignited by a voltaic spark in oxygen in a small tube over lime-water ; it seemed to give a slight opalescence to the liquid, but the quantity was so small that the experiment was not to be relied on. No definite change was perceptible in the bromine ; it seemed to be a little darker in colour and had a few black specks floating in it, which I judged to be minute portions of the same crust which had formed on the platinum wire, and which had become detached.

The experiment was repeated with chloride of iodine and with the same result, except that the quantity of oxygen was greater : I collected as much as half a cubic inch in some experiments. From an equal quantity of chloride of iodine, the platinum wire, however, was more quickly acted on than with the bromine, and the glass of the tube around it to some extent.

Melted phosphorus was exposed to the heat of the voltaic disruptive discharge by taking this between platinum points in a tube of phosphorus, similarly to an experiment of Davy's, but with better means of experimenting ; a considerable quantity of phosphuretted hydrogen was given off, amounting in several experiments to more than a cubic inch.

A similar experiment was made with melted sulphur, and sulphuretted hydrogen was given off, but not in such quantities as the phosphuretted hydrogen. I tried in vain to carry on these experiments beyond a certain point ; the substance became pasty, mixed with platinum from the arc, and from the difficulty of working with the same freedom as when they were fresh, the glass tubes were always broken after a certain time. Had I time for working on the subject now, I should use the discharge from the Ruhmkorf coil, which had not been invented at the period of these experiments. At a subsequent period, when this discharge was taken in the vacuum receiver of an air-pump from a metallic point to a metallic capsule containing phosphorus, a considerable yellow deposit lined the receiver, which, on testing,

turned out to be allotropic phosphorus. No gas is, however, given off. I had an air-pump (described, 'Phil. Trans.,' 1852, p. 101) which enabled me to detect very small quantities of gas, but I could get none. It was in making these experiments that I first detected the striæ in the electric discharge, which have since become a subject of such interesting observations, which are seen, perhaps, more beautifully in this phosphorus vapour than in any other medium, and which cease, or become very feeble, where the allotropic phosphorus is not produced.

I tried also phosphorus highly heated by a burning-glass in an atmosphere of nitrogen, but could eliminate no perceptible quantity of gas, though the phosphorus was changed into the allotropic form.

It is not difficult to understand why gas is not perceptibly eliminated in the last two experiments; the effect is probably similar to that described in my paper on the "Decomposition of Water by Heat," where, when the arc or electric spark is taken in aqueous vapour, a minute bubble of oxyhydrogen gas is freed and disseminated through the vapour, recombination being probably prevented by this dilution; but, however long the experiment may be continued, no increased quantity of the gas is obtained, all beyond this minute quantity being recombined. If, however, the bubble of gas be collected, by allowing the vapour to cool, and then expelled, a fresh portion is decomposed, and so on.

So with the phosphorus in the experiments in the air-pump and with the burning-glass; if any gas is liberated it is probably immediately recombined with the phosphorus; possibly a minute residuum might escape recombination, but the circumstances of the experiment did not admit of this being collected, as the gas was with the aqueous vapour.

When, on the other hand, the gas freed is immediately cut off from the source of heat, as when the spark is taken in liquids, an indefinite quantity can be obtained.

Decomposition and the elimination of gas may thus take place by the application of intense heat to a point in a liquid, or also in gas or vapours; but, in the latter case, it is more likely to be masked by the quantity of gas or vapour through which it is disseminated.

I believe there are very few gases in which some alteration does not take place by the application of the intense heat of the voltaic arc or electric spark. If the arc be taken between platinum points in dry oxygen-gas over mercury, the gas diminishes indefinitely, until the mercury rises, and by reaching the point where the arc takes place, puts an end to the experiment. I have caused as much as a cubic inch of oxygen to disappear by this means. I at one time thought this was due to the oxidation of the platinum; but the high heat renders this improbable, and the deposit formed on the interior of the glass tube in which the experiment is made has all the properties of platinum-black; so if the spark from a Ruhmkorf coil be taken in the vapour of water for several days, a portion of gas is freed which is pure hydrogen, the

oxygen freed being probably changed into ozone, and dissolved by the water in this case, while in the former it combined with the mercury.

I have alluded to the eudiometer by which I analyzed the gases obtained in these experiments; it was formed simply of a tube of glass, frequently not above $2\frac{1}{2}$ millimetres in diameter, with a loop of wire hermetically sealed into one end, the other having an open bell-mouth. By a platinum wire a small bubble of the gas to be examined could be got up through water or mercury into the closed end of the tube, and by the addition of a bubble of oxygen or hydrogen gas, a very accurate analysis of very minute quantities of gas could be made: I have analyzed by this means quantities no larger than a partridge-shot.

I need hardly allude to results on the compound liquids, such as oils and hydrocarbons, as the fact that permanent gas is given off in boiling such liquids would not be unexpected; but the above experiments seem to show that boiling is by no means necessarily the phenomenon that has generally been supposed, *viz.* a separation of cohesion in the molecules of a liquid from distension by heat. I believe, from the close investigation I made into the subject, that (except with the metals, on which there is no evidence) no one has seen the phenomenon of pure boiling without permanent gas being freed, and that what is ordinarily termed boiling arises from the extrication of a bubble of permanent gas either by chemical decomposition of the liquid, or by the separation of some gas associated in minute quantity with the liquid, and from which human means have hitherto failed to purge it; this bubble once extricated, the vapour of the liquid expands it, or, to use the appropriate phrase of M. Donny, the liquid evaporates against the surface of the gas.

My experiments are, in a certain sense, the complement of his. He showed that the temperature of the boiling point was raised in some proportion as water was deprived of air, and that under such circumstances the boiling took place by *soubresauts*. I have, I trust, shown that when the vapour liberated by boiling is allowed to condense, it does not altogether collapse into a liquid, but leaves a residual bubble of permanent gas, and that at a certain point this evolution becomes uniform.

Boiling, then, is not the result of merely raising a liquid to a given temperature, it is something much more complex.

One might suppose that with a compound liquid the initial bubble by which evaporation is enabled to take place might, if all foreign gas were or could be extracted, be formed by decomposition of the liquid: but this could not be the case with an elementary liquid; whence the oxygen from bromine or the hydrogen from phosphorus and sulphur? As with the nitrogen in water, it may be that a minute portion of oxygen, hydrogen, or of water is inseparable from these substances, and that if boiled away to absolute dryness, a minute portion of gas would be left for each ebullition.

With water there seems a point at which the temperature of ebul-

lition and the quantity of nitrogen yielded become uniform, though the latter is excessively minute.

The circumstances of the experiments with bromine, phosphorus, and sulphur, did not permit me to push the experiment so far as was done with water, but as far as it went the result was similar.

When an intense heat, such as that from the electric spark or voltaic arc, is applied to permanent gas, there are, in the greater number of cases, signs either of chemical decomposition or of molecular change; thus compound gases, such as hydrocarbons, ammonia, the oxides of nitrogen, and many others are decomposed. Phosphorus in vapour is changed to allotropic phosphorus, oxygen to ozone, which, according to present experience, may be viewed as allotropic oxygen. There may be many cases where, as with aqueous vapour, a small portion only is decomposed, and this may be so masked by the volume of undecomposed gas as to escape detection; if, for instance, the vapour of water were incondensable, the fact that a portion of it is decomposed by the electric spark or ignited platinum would not have been observed.

All these facts show that the effect of intense heat applied to liquids and gases is much less simple, and presents greater interest to the chemist than has generally been supposed. In far the greater number of cases, possibly in all, it is not mere expansion into vapour which is produced by intense heat, but there is a chemical or molecular change. Had circumstances permitted I should have carried these experiments further, and endeavoured to find an *experimentum crucis* on the subject. There are difficulties with such substances as bromine, phosphorus, &c., arising from their action on the substances used to contain and heat them, which are not easy to vanquish, and those who may feel inclined to repeat my experiments will find these difficulties greater than they appear in narration; but I do not think they are insuperable, and hope that, in the hands of those who are fortunate enough to have time at their disposal, they may be overcome.

To completely isolate a substance from the surrounding air and yet be able to experiment on it, is far more difficult than is generally supposed. The air-pump is but a rude mode for such experiments as are here detailed.

Caoutchouc joints are out of the question; even platinum wires carefully sealed into glass, though, as far as I have been able to observe, forming a joint which will not allow gas to pass, yet it is one through which liquids will effect a passage, at all events when the wires are repeatedly heated.

In some experiments with the ignited platinum wire hermetically sealed into a tube of glass, the end of the tube containing the platinum wire was placed in a larger tube of oil, to lessen the risk of cracking the glass. After some days' experimenting, though the sealing remained perfect, a slight portion of carbon was found in the interior liquid. This does not affect the results of my experiments, as I repeated them with glass tubes closed at the end and without platinum wires, and also without the oil-bath; but it shows how difficult it is to exclude sources

of error. When water has been deprived of air to the greatest practicable extent it becomes very avid for air. The following experiment is an instance of this: A single pair of the gas-battery, the liquid in which was cut off from the external air by a greased glass stopper, having one tube filled with water, the other with hydrogen, the platinized platinum plates in each of these tubes were connected with a galvanometer, and a deflection took place from the reaction of the hydrogen on the air dissolved in the water. After a time the deflection abated, and the needle returned to zero, all the oxygen of the air having become combined with the hydrogen. If now the stopper were taken out, a deflection of the galvanometric needle immediately took place, showing that the air rapidly enters the water as water would a sponge. Absolute chemical purity in the ingredients is a matter, for refined experiments, almost unattainable; the more delicate the test, the more some minute residual product is detected; it would seem (to put the proposition in a somewhat exaggerated form) that in nature everything is to be found in anything if we carefully look for it.

I have indicated the above sources of error to show the close pursuit that is necessary when looking for these minute residual phenomena. Enough has, I trust, been shown in the above experiments to lead to the conclusion that, hitherto, simple boiling, in the sense of a liquid being expanded by heat into its vapour without being decomposed or having permanent gas eliminated from it, is a thing unknown. Whether such boiling *can* take place may be regarded as an open question, though I incline to think it cannot; that if water, for instance, could be absolutely deprived of nitrogen, it would not boil until some portion of it was decomposed; that the physical severance of the molecules by heat is also a chemical severance. If there be anything in this theoretic view, there is great promise of important results on elementary liquids, if the difficulties to which I have alluded can be got over.

The constant appearance of nitrogen in water, when boiled off out of contact with the air almost to the last drop, is a matter well worthy of investigation. I will not speculate on what possible chemical connection there may be between air and water; the preponderance of these two substances on the surface of our planet, and the probability that nitrogen is not the inert diluent in respiration that is generally supposed, might give rise to not irrational conjectures on some unknown bond between air and water. But it would be rash to announce any theory on such a subject; better to test any guess one may make, by experiment, than to mislead by theory without sufficient data, or to lessen the value of facts by connecting them with erroneous hypotheses.

[W. R. G.]

Friday, February 12, 1864.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. Vice-President,
in the Chair.

J. ALFRED WANKLYN, Esq.

PROFESSOR OF CHEMISTRY, LONDON INSTITUTION,

On the Synthesis of Organic Bodies.

ON this tray you will see a collection of well-known substances.* Compare these substances with one another, and you will be struck with their dissimilarities. Some are solids and crystalline and brittle, others are liquids which are more fluid than water. Some are without colours; others are highly coloured, and are used for dyeing. Some are sweet, others are bitter; some have delightful perfumes, others have dreadful smells; some are wholesome food, others the most powerful poisons known to man.

In spite of this wonderful diversity in their properties, all the specimens on this tray are compounds of carbon, with a very few elements. Carbon, hydrogen, oxygen, and nitrogen are the only elements which occur in this collection of substances. Some of these substances contain carbon and hydrogen; some contain carbon, hydrogen, and oxygen; some, carbon, hydrogen, and nitrogen, and some again contain carbon, hydrogen, oxygen, and nitrogen. But not one of the specimens on this tray contains anything besides these four elements.

There is no difficulty in resolving any one of these substances into its ultimate elements. This sugar,† for example, on being heated to redness in a tube, leaves a black deposit which is carbon, whilst a liquid which is water distils over. If we were to electrolyse this liquid, we should obtain hydrogen and oxygen, and so we should exhibit carbon, hydrogen, and oxygen obtained from sugar. Again, instead of heating this sugar in the tube without allowing the air free access to it, we might burn it in excess of oxygen. If we were to do so, we should obtain carbonic acid and water, and, moreover, all the carbon in the sugar would assume the form of carbonic acid, and all the hydrogen the form of water. So we can obtain carbon and hydrogen, either in the free state, or in the very common and well-known forms of combination as carbonic acid and water. Nitrogen, when it is present, can be made to assume the form of free nitrogen. For that purpose, all that is requisite is to heat the substance to red-

* A tray, with a number of organic bodies lying upon it, was before the speaker.

† Cane sugar was heated to redness in a tube.

ness with excess of oxygen, and to adopt certain precautions to avoid the production of oxide of nitrogen.

Thus, the pulling to pieces of these substances on the tray is a matter of very little difficulty: more than fifty years ago chemists could do that—but how to put the pieces together again is a much more difficult task.

Sugar consists of 72 parts by weight of carbon, 11 parts of hydrogen, and 88 parts of oxygen. We may bring together carbon, hydrogen, and oxygen in these proportions, and shake them up together, or heat them or cool them, and yet we shall never get them to combine so as to form sugar. Alcohol consists of 24 parts of carbon, 6 parts of hydrogen, and 16 parts of oxygen, but no alcohol ever results from making such a mixture. Neither sugar nor alcohol can exist at the temperature to which it is requisite to raise our mixture of carbon, hydrogen, and oxygen, in order to get chemical action to set in. At ordinary temperature the organic elements will not enter into combination, whilst at high temperatures they combine, it is true, but yield comparatively very few compounds.

It was long after chemists had effected the analysis of organic bodies before they learnt how to effect the synthesis of even one of them, and hence the belief sprung up that organic products, such as those on our tray, were intrinsically different from mineral products. Whilst stones, water, and the like were regarded as having their ultimate particles held together by mere dead forces, sugar, alcohol, &c., were regarded as being held together by vital forces, as being in short, in some subordinate way, alive.

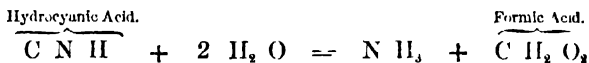
Now, no more positive refutation of this notion can be imagined than the artificial construction of substances, in every respect, like those obtained from the animal and vegetable kingdoms, and hence some of the philosophical interest attached to the problem which forms the subject of this discourse.

The first definite example of the construction of an organic body from inorganic materials was given by Wöhler, in 1828, when he made the organic base urea from cyanate of ammonia.

Let us trace the steps of this process. Cyanide of potassium—a body which can exist at a red heat (some cyanide of potassium was exhibited in the form of tabular masses which had been fused), and which can moreover be formed directly from its constituents (carbon, nitrogen, and potassium)—was oxydized by means of peroxide of manganese at a low red heat, and so cyanate of potash was obtained. The cyanate of potash was next converted into cyanate of ammonia by double decomposition with sulphate of ammonia. Thus cyanate of ammonia was produced from its elements by a process which, although indirect, still did not involve the action of either a plant or an animal. Cyanate of ammonia becomes urea, when its solution in water is simply evaporated to dryness.

It was curious that the first organic body to be constructed should have been a nitrogenous compound.

In 1831, three years after this important discovery of Wöhler's, formic acid—the first term of the fatty acid series—was obtained from inorganic materials by Pelouze. The process was this:—Hydrocyanic acid, a body capable of being obtained from inorganic materials, was heated either with strong alkalies or acids, and was so made to react upon the elements of water as follows:—



and yielded formic acid.

It does not appear that this research of Pelouze's attracted that attention which it deserved. This we must attribute to the circumstance, that at this period the position of formic acid in the organic series was not recognized.

The next step of importance in organic synthesis was taken by Kolbe in 1845. It was the synthesis of acetic acid, the second term of the fatty series. Kolbe's process was this:—Sulphide of carbon, obtained by the direct combination of carbon with sulphur at a red heat, was submitted to the action of chlorine at a red heat, by which means certain compounds of carbon and chlorine were obtained. One of the compounds, $\text{C}_2 \text{ Cl}_4$, was then acted upon by chlorine in the presence of water, and tri-chlor-acetic acid resulted.

Having thus got tri-chlor-acetic acid by thoroughly inorganic means, Kolbe availed himself of the observation which had been made of *Melsens*—that treatment of tri-chlor-acetic acid with potassium-amalgam and water converted it into acetic acid.

Kolbe was fully sensible of the scope and importance of his discovery. The following passage occurs in his paper, published in 'Liebig's Annalen,' for 1845:—"From the foregoing observations we deduce the interesting fact that acetic acid, hitherto known only as a product of the oxidation of organic materials, can be built up by almost direct synthesis from its elements. Sulphide of carbon, chloride of carbon, and chlorine are the agents which, along with water, accomplish the transformation of carbon into acetic acid. If we could only transform acetic acid into alcohol, and out of the latter could obtain sugar and starch, then we should be enabled to build up these common vegetable principles, by the so-called artificial method, from their most ultimate elements." Thus it appears that Kolbe looked forward to the building up of organic bodies in general, and that he was quite alive to the fact that the synthesis of acetic acid completed the synthesis of the derivations of acetic acid.

Among these derivations may be enumerated acetone, the product of the destructive distillation of acetates: marsh gas, obtained by distilling an acetate with a caustic alkali. ethylene, obtained by Bunsen by heating kakodyl, which itself results by the action of arsenious acid upon an acetate. The electrolysis of acetic acid, which Kolbe accomplished a few years afterwards, yielded methyl and

oxide of methyl, which latter, in its turn, could be transformed into any other methylic compound.

Marsh gas was moreover prepared by Regnault, by treating C Cl_4 with nascent hydrogen; and the common methylic compounds appear to have been produced by Dumas from marsh gas, the chloride of methyl having been obtained by Dumas by the action of chlorine upon marsh gas.

Before 1854, all the foregoing syntheses were fully completed, *i. e.* there was no step missing between the elements themselves and the most complex compound reached; but, in addition to these complete and definite syntheses, there had also been a good deal of building up of an incomplete or of a less definite character before 1854.

It was known, in a general way, that organic bodies of tolerably simple composition sometimes gave complex products on destructive distillation. Thus, alcohol was known to give naphthaline, benzol, and carbonic acid when it was pressed through a red-hot tube. Formiates were also known to yield hydro-carbons when they were subjected to destructive distillation. The precise dates of these different observations I cannot give, but hand-books of chemistry, published before 1854, contain a statement of the facts.

A few years after 1820—before Wöhler's celebrated Synthesis of Urea, a very remarkable instance of passage from a simpler to a more complex compound was given by Faraday and Hennell. This example is placed along with the indefinite syntheses because it was generally disbelieved in by chemists, and only within the last few years, when it was confirmed by Berthelot, received the general assent. Faraday and Hennell found that olefiant gas was absorbed by sulphuric acid and gave sulpho-vinic acid, from which of course alcohol and the ethers might be procured. Liebig denied what Faraday and Hennell had asserted, and the latter did not insist upon the correctness of their work, and did not take the necessary steps for ensuring the reception of their results.

Shortly before 1854, a most capital addition to the art of organic synthesis was borrowed from the doctrine of the Homologous Series. I will endeavour to explain it.

Organic bodies repeat themselves: thus common alcohol has a whole series of representatives, differing from it in formula by $n(\text{C H}_2)$, but resembling it very closely in chemical functions. Alcohol, and these its representatives, constitute a homologous series. Every one of these representatives (homologues) of alcohol possesses a set of ethers and other derivatives, just as common alcohol possesses its ethers and derivatives. With certain limitations, it is true that whatever reaction can be accomplished with one alcohol can be accomplished with any other alcohol of the series.

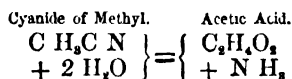
Synthesis by series will then be easily understood by an example:—Suppose we obtain a building-up by starting with common alcohol, we should infer that an analogous building-up could be made by starting with any other alcohol of the series.

Here follows a table of the homologous series of alcohols, and of

the homologous acids which are related to them :—

Methyl alc.	C_1H_4O	$C_1H_2O_2$	Formic Acid
Ethyl alc.	C_2H_6O	$C_2H_4O_2$	Acetic A.
Propyl alc.	C_3H_8O	$C_3H_6O_2$	Propionic A.
Tetrl alc.	$C_4H_{10}O$	$C_4H_8O_2$	Butyric A.
Amyl alc.	$C_5H_{12}O$	$C_5H_{10}O_2$	Valerianic A.
Hexyl alc.	$C_6H_{14}O$	$C_6H_{12}O_2$	Caproic A.
Cetyl alc.	$C_{16}H_{34}O$	$C_{16}H_{32}O_2$	Palmitic A.
Ceryl alc.	$C_{27}H_{56}O$	$C_{27}H_{54}O_2$	Cerotic A.

A good example of synthesis by series was furnished by Frankland and Kolbe, who showed that various cyanides of the alcohol-radicles yield the next higher acid in the series when they are digested with an alcoholic solution of potash, thus :—



The effect of the alkali is to cause decomposition of water by means of the cyanide, and the reaction very closely resembles Pelouze's, of which mention has already been made.

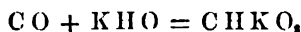
By means of this synthesis, which is general to the whole series, chemists acquired a method of ascending from any given alcohol to the acid belonging to next higher alcohol. It will be evident, however, that this step, important though it was, did not suffice to enable chemists to march regularly up the ladder. The step from acetic acid to alcohol—from an acid to an alcohol of the same carbon-condensation, was wanting.

This synthesis by series was an incomplete synthesis; there was a gap requiring to be filled up, in order that the regular march might be made up the vinic series.

From the foregoing, it will be seen that by the year 1854 very considerable progress had been made in the building-up of organic bodies from their ultimate elements.

We now pass on to the consideration of the period comprising the last ten years, from 1854 up to the present time.

During this period we have had new methods of accomplishing some of the syntheses which had been effected previously. Thus, formic acid, which, as we have seen, had been formed from inorganic materials so long ago as 1831, was built up by Berthelot by means of carbonic oxide and caustic potash,—



and again by Kolbe, by using carbonic acid, moisture, and sodium (the moisture and sodium giving nascent hydrogen),—



Again also, the passage from an alcohol to the next higher acid was repeated. Carbonic acid and a compound of an alcohol-radicle with an alkali-metal coalesced, and formed a salt of a fatty acid thus:—

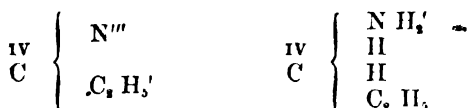


Still these reactions, however interesting they might be, were not new syntheses; they were only new methods of effecting old syntheses.

The great problem, how to step from one alcohol to that next above it, has received a general solution from Mendius. Mendius used cyanogen compounds, those hydrocyanic ethers which had already done such good service to organic synthesis, and exposed them to the action of nascent hydrogen, and so obtained amides of alcohol-radicles higher than the alcohol-radicles started from. The reaction bears a close similarity to the one which takes place between the cyanides and alcoholic solution of potash, and which, as will be remembered, enabled us to pass from the alcohol to the acid next above.

Here is a scheme to show the parallel:—

TRANSFORMATION TO AMIDE.



TRANSFORMATION TO ACID.



In the one case nitrogen is replaced by N H_2 and H , H ; and in the other by $\text{H O}'$ and O'' .

Mendius was able to commence even with hydrocyanic acid. The steps in his synthesis are these:—

HYDROCYANIC ACID TO METHYLAMINE.



* The experiment was shown, and the great evolution of heat which took place on bringing carbonic acid into contact with sodium-ethyl was apparent.

Methylamine, by means of nitrous acid to methyl alcohol: methyl-alcohol to cyanide of methyl, well-known processes being employed to effect this:

CYANIDE OF METHYL TO ETHYLAMINE.

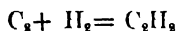


From ethylamine it is easy to get cyanide of ethyl, from which, by a third repetition, we arrive at the propylic stage:—



Thus the vinic series may be ascended; thus there is reason to think we may begin with so simple a body as prussic acid, and step by step proceed from one alcohol to the next above it, until we reach the fats and the waxes. There are other methods of effecting the synthesis of the alcohol series, but none of them seem to be so complete and satisfactory as this:—Berthelot has obtained alcohols by adding the elements of water to the olefines, and some of the olefines he has obtained by the destructive distillation of formiates; but it is an open question—how many olefines can be got by heating the formiates? And, at any rate, there is no precision in the preparation of olefines from formiates.

A very neat and beautiful way of preparing one olefine, *viz.* common olefant gas is, however, due to Berthelot. He exposes charcoal to the action of hydrogen at a very high temperature—the temperature of the electric arc, and then union takes place, resulting in the formation of Acetylene:



Acetylene exposed to the action of nascent hydrogen in an alkaline liquid gives olefant gas:—



Friedel and Wurtz have converted aldehydes and ketones into alcohols by the action of nascent hydrogen, and thence there arises another method of ascending the vinic series, and besides there are a number of other reactions which are capable of more or less general employment for the purpose of building up the alcoholic series, but which we have not time to particularize.

The alcohols having been got, many other important organic compounds follow, and there is good reason for believing that with the progress of the science all will be derived from them, so that the series of the alcohols will constitute a kind of backbone to organic chemistry.

Most modern organic researches are capable of being looked at from a synthetical aspect, for they generally disclose how to devise some organic bodies from compounds which either themselves are, or will be,

capable of complete synthesis. Glycerine, the base of the fats, has been derived from the propylic series, having been obtained, by Wurtz, by a somewhat circuitous process from propylene—the olefine of that series.

The sugars have not been, as yet, *unequivocally* produced, but they will be, for their connection with the hexylic series is now placed beyond a doubt. The production of glycerides from glycerine and fatty acids is the proof that the natural fats are within our grasp. The aromatic series with its many derivations, among which may be mentioned the wonderful aniline dyes which rival those got more immediately from the animal and vegetable kingdoms, becomes accessible to synthesis through common alcohol, which on being heated to redness gives benzol and carbolic acid—members of the aromatic series.

Wurtz's compound ammonias, and above all, the immense and wonderful development of the class of compound ammonias arising from the labours of Hofmann are the pledge that the natural alkaloids—quinine, morphine, strychnine, and their congeners will one day be within our reach.

Glycocoll, produced by Perkin and Duppa from acetic acid, and the bases of the juice of flesh, which have been recently formed by Vollhardt and Hofmann, assure us that albumen—that essential ingredient of our food—will not elude us.

Why should those medicines and foods which we find in nature be the most useful which are possible? Would it not rather be strange if they were?

Hereafter, perhaps, medicines as much more potent than quinine, as quinine is than the extracts of the commonest herb that grows wild, may be the produce of our laboratories.

[J. A. W.]

Friday, March 4, 1864.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. Vice-President,
in the Chair.

PROFESSOR G. G. STOKES, M.A. D.C.L. SEC. R.S.

On the Discrimination of Organic Bodies by their Optical Properties.

THE chemist who deals with the chemistry of inorganic substances has ordinarily under his hands bodies endowed with very definite reactions, and possessing great stability, so as to permit of the employment of energetic reagents. Accordingly he may afford to dispense with the aids supplied by the optical properties of bodies, though even to him they might be of material assistance. The properties alluded to are such as can be applied to the scrutiny of organic substances; and therefore the examination of the bright lines in flames and incandescent vapours is not considered. This application of optical observation, though not new in principle (for it was clearly enunciated by Mr. Fox Talbot more than thirty years ago), was hardly followed out in relation to chemistry, and remained almost unknown to chemists until the publication of the researches of Professors Bunsen and Kirchhoff, in consequence of which it has now become universal.

But while the chemist who attends to inorganic compounds may confine himself without much loss to the generally-recognized modes of research, it is to his cost that the organic chemist, especially one who occupies himself with proximate analysis, neglects the immense assistance which in many cases would be afforded him by optical examination of the substances under his hands. It is true that the method is of limited application, for a great number of substances possess no marked optical characters; but when such substances do present themselves, their optical characters afford facilities for their chemical study of which chemists generally have at present little conception.

Two distinct objects may be had in view in seeking for such information as optics can supply relative to the characters of a chemical substance. Among the vast number of substances which chemists have now succeeded in isolating or preparing, and which in many cases have been but little studied, it often becomes a question whether two substances, obtained in different ways, are or are not identical. In such cases an optical comparison of the bodies will either add to the evidence of their identity, the force of the additional evidence being greater or less according as their optical characters are more or less marked, or will establish a difference between substances which might otherwise erroneously have been supposed to be identical.

The second object is that of enabling us to follow a particular substance through mixtures containing it, and thereby to determine its principal reactions before it has been isolated, or even when there is small hope of being able to isolate it; and to demonstrate the existence of a common proximate element in mixtures obtained from two different sources. Under this head should be classed the detection of mixtures in what were supposed to be solutions of single substances.*

Setting aside the labour of quantitative determinations carried out by well-recognized methods, the second object is that the attainment of which is by far the more difficult. It involves the methods of examination required for the first object, and more besides; and it is that which is chiefly kept in view in the present discourse.

The optical properties of bodies, properly speaking, include every relation of the bodies to light; but it is by no means every such relation that is available for the object in view. Refractive power, for instance, though constituting, like specific gravity, &c., one of the characters of any particular pure substance, is useless for the purpose of following a substance in a mixture containing it. The same may be said of dispersive power. The properties which are of most use for our object are—first, absorption; and secondly, fluorescence.

Colour has long been employed as a distinctive character of bodies; as, for example, we say that the salts of oxide of copper are mostly blue. The colour, however, of a body, gives but very imperfect information respecting that property on which the colour depends; for the same tint may be made up in an infinite number of ways from the constituents of white light. In order to observe what it is that the body does to each constituent, we must examine it in a pure spectrum. [The formation of a pure spectrum was then explained, and such a spectrum was formed on a screen by the aid of the electric light. On holding a cell containing a salt of copper in front of the screen, and moving it from the red to the violet, it was shown to cast a shadow in the red as if the fluid had been ink, while in the blue rays it might have been supposed to have been water. Chromate of potash similarly treated gave the reverse effect, being transparent in the red and opaque in the blue. Of course the transition from transparency to opacity was not abrupt; and for intermediate colours the fluids caused a partial darkening. Indeed, to speak with mathematical rigour, the darkening is not absolute even when it appears the greatest; but the light let through is so feeble that it eludes our senses. In this way the behaviour of the substance may be examined with reference to the various kinds of light one after another; but in order to see at one glance its behaviour with respect to all kinds, it is merely requisite to hold the body so as to intercept the whole beam which forms the spectrum, to place it, for instance, immediately in front of the slit.]

* The detection of mixtures by the microscopic examination of intermingled crystals properly belongs to the first head, the question which the observer proposes to himself, being, in fact, whether the pure substances forming the individual crystals are or are not identical.

To judge from the two examples just given, it might be supposed that the observation of the colour would give almost as much information as analysis by the prism. To show how far this is from being the case, two fluids very similar in colour, port-wine and a solution of blood, were next examined. The former merely caused a general absorption of the more refrangible rays; the latter exhibited two well-marked dark bands in the yellow and green. These bands, first noticed by Hoppe, are eminently characteristic of blood, and afford a good example of the facilities which optical examination affords for following a substance which possesses distinctive characters of this nature. On adding to a solution of blood a particular salt of copper (any ordinary copper salt, with the addition of a tartrate to prevent precipitation, and then carbonate of soda), a fluid was obtained utterly unlike blood in colour, but showing the characteristic bands of blood, while at the same time a good deal of the red was absorbed, as it would have been by the copper salt alone. On adding, on the other hand, acetic acid to a solution of blood, the colour was merely changed to a browner red, without any precipitate being produced. Nevertheless, in the spectrum of this fluid the bands of blood had wholly vanished, while another set of bands less intense, but still very characteristic, made their appearance. This alone, however, does not decide whether the colouring matter is decomposed or not by the acid; for as blood is an alkaline fluid, the change might be supposed to be merely analogous to the reddening of litmus. To decide the question, we must examine the spectrum when the fluid is again rendered alkaline, suppose by ammonia, which does not affect the absorption bands of blood. The direct addition of ammonia to the acid mixture causes a dense precipitate, which contains the colouring matter, which may, however, be separated by the use merely of acetic acid and ether, of which the former was already used, and the latter does not affect the colouring matter of blood. This solution gives the same characteristic spectrum as blood to which acetic acid has been added; but now there is no difficulty in obtaining the colouring matter in an ammoniacal solution. In the spectrum of this solution, the sharp absorption bands of blood do not appear, but instead thereof there is a single band a little nearer to the red, and comparatively vague [this was shown on a screen]. This difference of spectra decides the question, and proves that hæmatin (the colouring matter prepared by acid, &c.) is, as Hoppe stated, a product of decomposition.

The spectrum of blood may be turned to account still further in relation to the chemical nature of that substance. The colouring matter contains, as is well known, a large quantity of iron; and it might be supposed that the colour was due to some salt of iron, more especially as some salts of peroxide of iron, sulphocyanide for instance, have a blood-red colour. But there was found a strong general resemblance between salts of the same metallic oxide as regards the character of their absorption. Thus the salts of sesquioxide of uranium show a remarkable system of bands of absorption in the more refran-

gible part of the spectrum. The number and position of the bands differ a little from one salt to another ; but there is the strongest family likeness between the different salts. Salts of sesquioxide of iron in a similar manner have a family likeness in the vagueness of the absorption, which creeps on from one part of the spectrum to another without presenting any rapid transitions from comparative transparency to opacity and the converse. [The spectrum of sulphocyanide of peroxide of iron was shown for the sake of contrasting with blood.] Hence the appearance of such a peculiar system of bands of absorption in blood would negative the supposition that its colour is due to a salt of iron as such, even had we no other means of deciding. The assemblage of the facts with which we are acquainted seems to show that the colouring matter is some complex compound of the five elements, oxygen, hydrogen, carbon, nitrogen, and iron, which under the action of acids and otherwise, splits into hæmatin and a protein substance.

This example was dwelt on, not for its own sake, but because general methods are most readily apprehended in their application to particular examples. To show one example of the discrimination which may be effected by the prism, the spectra were exhibited of the two kinds of red glass which (not to mention certain inferior kinds) are in common use, and which are coloured, one by gold, and the other by suboxide of copper. Both kinds exhibit a single band of absorption near the yellow or green ; but the band of the gold glass is situated very sensibly nearer to the blue end of the spectrum than that of the copper glass.

In the experiments actually shown, a battery of fifty cells and complex apparatus were employed, involving much trouble and expense. But this was only required for projecting the spectra on a screen, so as to be visible to a whole audience. To see them, nothing more is required than to place the fluid to be examined (contained, suppose) in a test tube, behind a slit, and to view it through a small prism applied to the naked eye, different strengths of solution being tried in succession. In this way the bands may be seen by anyone in far greater perfection than when, for the purpose of a lecture, they are thrown on a screen.

In order to be able to examine the peculiarities which a substance may possess in the mode in which it absorbs light, it is not essential that the substance should be in solution, and viewed by transmission. Thus, for example, when a pure spectrum is thrown on a sheet of paper painted with blood, the same bands are seen in the yellow and green region as when the light is transmitted through a solution of blood, and the spectrum thrown on a white screen. This indicates that the colour of such a paper is in fact due to absorption, although the paper is viewed by reflected light. Indeed, by far the greater number of coloured objects which are presented to us, such as green leaves, flowers, dyed cloths, though ordinarily seen by reflection, owe their colour to absorption. The light by which they are seen is, it is true, reflected, but it is not *in reflection* that the preferential selection of certain kinds of rays is made which causes the objects to appear coloured.

Take, for example, red cloth. A small portion of the incident light is reflected at the outer surfaces of the fibres, and this portion, if it could be observed alone, would be found to be colourless. The greater part of the light penetrates into the fibres, when it immediately begins to suffer absorption on the part of the colouring matter. On arriving at the second surface of the fibre, a portion is reflected and a portion passes on, to be afterwards reflected from, or absorbed by, fibres lying more deeply. At each reflection the various kinds of light are reflected in as nearly as possible the same proportion; but in passing across the fibres, in going and returning, they suffer very unequal absorption on the part of the colouring matter, so that in the aggregate of the light perceived the different components of white light are present in proportions widely different from those they bear to each other in white light itself, and the result is a vivid colouring.

There are, however, cases in which the different components of white light are reflected with different degrees of intensity, and the light becomes coloured by regular reflection. Gold and copper may be referred to as examples. In ordinary language we speak of a soldier's coat as red, and gold as yellow. But these colours belong to the substances in two totally different senses. In the former case the colouring is due to absorption, in the latter case to reflection. In the same sense, physically speaking, in which a soldier's coat is red, gold is not yellow but blue or green. Such is, in fact, the colour of gold by transmission, and therefore as the result of absorption, as is seen in the case of gold leaf, which transmits a bluish green light, or of a weak solution of chloride of gold after the addition of protosulphate of iron, when the precipitated metallic gold remains in suspension in a finely-divided state, and causes the mixture to have a blue appearance when seen by transmitted light. In this case we see that while the substance copiously reflects and intensely absorbs rays of all kinds, it more copiously reflects the less refrangible rays, with respect to which it is more intensely opaque.

All metals are, however, highly opaque with regard to rays of all colours. But certain non-metallic substances present themselves which are at the same time intensely opaque with regard to one part of the spectrum, and only moderately opaque or even pretty transparent with regard to another part. Carthamine, murexide, platino-cyanide of magnesium may be mentioned as examples. Such substances reflect copiously, like a metal, those rays with respect to which they are intensely opaque, but more feebly, like a vitreous substance, those rays for which they are tolerably transparent. Hence, when white light is incident upon them the regularly-reflected light is coloured, often vividly, those colours preponderating which the substance is capable of absorbing with intense avidity. But perhaps the most remarkable example known of the connection between intense absorption and copious reflection occurs in the case of crystals of permanganate of potash. These crystals have a metallic appearance, and reflect a greenish light. They are too dark to allow the transmitted light to be examined;

and even when they are pulverized, the fine purple powder they yield is too dark for convenient analysis of the transmitted light. But the splendid purple solution which they yield may be diluted at pleasure, and the analysis of the light transmitted by it presents no difficulty. The solution absorbs principally the green part of the spectrum; and when it is not too strong, or used in too great thickness, five bands of absorption, indicating minima of transparency, make their appearance [these were shown on a screen]. Now, when the green light reflected from the crystals is analyzed by a prism, there are observed *bright* bands, indicating maxima of reflecting power, corresponding in position to the *dark* bands in the light transmitted by the solution. The fifth bright band, indeed, can hardly, if at all, be made out, but the corresponding dark band is both less strong than the others and occurs in a fainter part of the spectrum. When the light is reflected at a suitable angle, and is analyzed both by a Nicol's prism, placed with its principal section in the plane of incidence, and by an ordinary prism, the whole spectrum is reduced to the bands just mentioned. The Nicol's prism would under these circumstances extinguish the light reflected from a vitreous substance, and transmit a large part of the light reflected from a metal. Hence we see that as the refrangibility of the light gradually increases, the substance changes repeatedly, as regards the character of its reflecting power, from vitreous to metallic and back again, as the solution (and therefore it may be presumed the substance itself) changes from moderately to intensely opaque, and conversely.

These considerations leave little doubt as to the chemical state of the copper present in a certain glass which was exhibited. This glass was coloured only in a very thin stratum on one face. By transmission it cut off a great deal of light, and was bluish. By reflection, especially when the colourless face was next the eye, it showed a reddish light visible in all directions, and having the appearance of coming from a fine precipitate, though it was not resolved by the microscope, at least with the power tried. It evidently came from a failure in an attempt to make one of the ordinary red glasses coloured by suboxide of copper, and the only question was as to the state in which the copper was present. It could not be oxide, for the quantity was too small to account for the blueness, and in fact the glass became sensibly colourless in the outer flame of a blowpipe. Analysis of the transmitted light by the prism showed a small band of absorption in the place of the band seen in those copper-red glasses which are not too deep, and therefore a small portion of copper was present in the state of suboxide, *i.e.* a silicate of that base. The rest was doubtless present as metallic copper, arising from over-reduction in the manufacture, and accordingly the blue colour, which would have been purer if the suboxide had been away, indicates the true colour of copper by transmitted light, quite in conformity with what we have seen in the case of gold. Hence, in both metals alike, the absorbing and the reflecting powers are, on the whole, greater for the less than for the more refrangible colours, the

law of variation with refrangibility being of course somewhat different in the two cases.

Time would not permit of more than a very brief reference to the second property to which the speaker had referred as useful in tracing substances in impure solutions—that of fluorescence. The phenomenon of fluorescence consists in this, that certain substances, when placed in rays of one refrangibility, emit during the time of exposure compound light of lower refrangibility. When a pure fluorescent substance (as distinguished from a mixture) is examined in a pure spectrum, it is found that on passing from the extreme red to the violet and beyond, the fluorescence commences at a certain point of the spectrum, varying from one substance to another, and continues from thence onwards, more or less strongly in one part or another according to the particular substance. The colour of the fluorescent light is found to be nearly constant throughout the spectrum. Hence, when in a solution presented to us, and examined in a pure spectrum, we notice the fluorescence taking, as it were, a fresh start, *with a different colour*, we may be pretty sure that we have to deal with a mixture of two fluorescent substances.

It might be inferred *à priori*, that fluorescence at any particular part of the spectrum would necessarily be accompanied by absorption, since otherwise there would be a creation of *vis rira*; and experience shows that rapid absorption (such as corresponds to a well-marked minimum of transparency indicated by a determinate band of absorption in the transmitted light) is accompanied by copious fluorescence. But experience has hitherto also shown, what could not have been predicted, and may not be universally true,* that conversely, absorption is accompanied, in the case of a fluorescent substance, by fluorescence.

From what precedes it follows that the colour of the fluorescent light of a solution, even when the incident light is white, or merely sifted by absorption, may be a useful character. To illustrate this, the electric light, after transmission through a deep-blue glass, was thrown on solutions in weak ammonia of two crystallized substances, *æsculin* and *fraxin*, obtained from the bark of the horse-chesnut, and of which the latter occurs also in the bark of the ash, in which, indeed, it was first discovered. Both solutions exhibited a lively fluorescence; but the colour was different, being blue in the case of *æsculin*, and bluish-

* Fluorescent substances, like others, doubtless absorb the invisible heat-rays lying beyond the extreme red, in a manner varying from one substance to another. Hence, if we include such rays in the incident spectrum, we have an example of absorption not accompanied by fluorescence. But the invisible heat-rays differ from those of the visible spectrum (as there is every reason to believe) only in the way that the visible rays of one part of the spectrum differ from those of another, that is, by wave length, and consequently by refrangibility, which depends on wave length. Hence it is not improbable that substances may be discovered which absorb the visible rays in some parts of the spectrum less refrangible than that at which the fluorescence commences; and *mixtures* possessing this property may be made at pleasure. Nevertheless, the speaker has not yet met with a pure fluorescent substance which exhibits this phenomenon.

green in the case of fraxin. A purified solution obtained from the bark exhibits a fluorescence of an intermediate colour, which would suffice to show that *æsculin* would not alone account for the fluorescence of the solution of the bark.

When a substance possesses well-marked optical properties, it is in general nearly as easy to follow it in a mixture as in a pure solution. But if the problem which the observer proposes to himself be :—Given a solution of unknown substances which presents well-marked characters with reference to different parts of the spectrum, to determine what portion of these characters belongs to one substance, and what portion to another, it presents much greater difficulties. It was with reference to this subject that the second of the objects mentioned at the beginning of the discourse had been spoken of as that the attainment of which was by far the more difficult. The problem can, in general, be solved only by combining processes of chemical separation, especially fractional separation, with optical observation. When a solution has thus been sufficiently tested, those characters which are found always to accompany one another, in, as nearly as can be judged, a constant proportion, may, with the highest probability, be regarded as belonging to one and the same substance. But while a combination of chemistry and optics is in general required, important information may sometimes be obtained from optics alone. This is especially the case when one at least of the substances present is at the same time fluorescent and peculiar in its mode of absorption.

To illustrate this the case of chlorophyll was referred to. An eminent French chemist, M. Fremy, proposed to himself to examine whether the green colour were due to a single substance, or to a mixture of a yellow and a blue substance. By the use of merely neutral bodies, he succeeded in separating chlorophyll into a yellow substance, and another which was green, but inclining a little to blue; but he could not in this way get further in the direction of blue. He conceived, however, that he had attained his object by dissolving chlorophyll in a mechanical mixture of ether and hydrochloric acid, the acid on separation showing a fine blue colour, while the ether was yellow. Now solutions of chlorophyll in neutral solvents, such as alcohol, ether, &c., show a lively fluorescence of a blood-red colour; and when the solution is examined in a pure spectrum, the red fluorescence, very copious in parts of the red, comparatively feeble in most of the green, is found to be very lively again in the blue and violet. Now a substance of a pure yellow colour, and exercising its absorption therefore, as such substances do, on the more refrangible rays, would not show a pure red fluorescence. Either it would be non-fluorescent, or the fluorescence of its solution would contain (as experience shows) rays of refrangibilities reaching, or nearly so, to the part of the spectrum at which the fluorescence, and therefore the absorption, commences; and therefore the fluorescent light could not be pure red, as that of chlorophyll is found to be even in the blue and violet. The yellow substance separated by M. Fremy, by the aid of neutral reagents, is, in fact,

non-fluorescent. Hence the powerful red fluorescence in the blue and violet can only be attributed to the substance exercising the well-known powerful absorption in the red, which substance must, therefore, powerfully absorb the blue and violet. We can affirm, therefore, *à priori*, that if this substance were isolated it would not be blue, but only a somewhat bluer green. The blue solution obtained by M. Fremy owes, in fact, its colour to a product of decomposition, which when dissolved in neutral solvents is not blue at all, but of a nearly neutral tint, showing, however, in its spectrum extremely sharp bands of absorption.

[G. G. S.]

Friday, March 18, 1864.

H.R.H. THE PRINCE OF WALES, Vice-Patron, in the Chair.

JOHN TYNDALL, Esq. F.R.S.

PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION.

Contributions to Molecular Physics.

THE speaker had already shown the enormous differences which exist among gaseous bodies, both as regards their power of absorbing and emitting radiant heat. When a gas is condensed to a liquid, or a liquid congealed to a solid, the molecules coalesce, and grapple with each other by forces which were insensible as long as the gaseous state was maintained. But, though the molecules are thus drawn together, the luminiferous ether still surrounds them: hence, if the acts of radiation and absorption depend on the individual molecules, they will assert their power even after the state of aggregation has been changed. If, on the contrary, their mutual entanglement by the force of cohesion be of paramount influence in the interception and emission of radiant heat, then we may expect that liquids will exhibit a deportment towards radiant heat altogether different from that of the vapours from which they are derived.

The first part of the present inquiry is devoted to an exhaustive examination of this question. The speaker employed twelve different liquids, and operated upon five different layers of each, which varied in thickness from 0.02 of an inch to 0.27 of an inch. The liquids were enclosed, not in glass vessels, which would have materially modified the heat, but between plates of transparent rock-salt, which but slightly affected the radiation. His source of heat throughout these comparative experiments consisted of a platinum wire, raised to incandescence by an electric current of unvarying strength. The quantities of radiant heat absorbed and transmitted by each of the liquids at the respective thicknesses were first determined. The vapours of these liquids were subsequently examined, the quantities of vapour employed being proportional to the quantities of liquid traversed by the radiant heat. The result of the comparison was that, for heat of the same quality, the order of absorption of liquids and of their vapours are identical. There was no exception to this law; so that, to determine the position of a vapour as an absorber or radiator, it is only necessary to determine the position of its liquid.

This result proves that the state of aggregation, as far at all events as the liquid stage is concerned, is of altogether subordinate moment—

a conclusion which will probably prove to be of cardinal importance in molecular physics. On one important and contested point it has a special bearing. If the position of a liquid as an absorber and radiator determine that of its vapour, the position of water fixes that of aqueous vapour. Water had been compared with other liquids in a multitude of experiments, and it was found that, as a radiant and as an absorbent, it transcends them all. Thus, for example, a layer of bisulphide of carbon 0.02 of an inch in thickness absorbs 6 per cent., and allows 94 per cent. of the radiation from the red-hot platinum spiral to pass through it; benzol absorbs 43 and transmits 57 per cent. of the same radiation; alcohol absorbs 67 and transmits 33 per cent., and alcohol stands at the head of all liquids except one in point of power as an absorber. The exception is *water*. A layer of this substance, of the thickness above given, absorbs 81 per cent., and permits only 19 per cent. of the radiation to pass through it. Had no single experiment ever been made upon the *vapour* of water, we might infer with certainty from the deportment of the *liquid* that, weight for weight, this vapour transcends all others in its power of absorbing and emitting radiant heat.

The relation of absorption and radiation to the chemical constitution of the radiant and absorbent substances was next briefly considered. For the first six substances in the list of those examined, the radiant and absorbent powers augment as the number of atoms in the compound molecule augments. Thus, bisulphide of carbon has 3 atoms, chloroform 5, iodide of ethyl 8, benzol 12, and amylene 15 atoms in their respective molecules; and the order of their power as radiants and absorbents is that here indicated; bisulphide of carbon being the feeblest, and amylene the strongest of the six. Alcohol, however, excels benzol as an absorber, though it has but 9 atoms in its molecule; but, on the other hand, its molecule is rendered more complex by the introduction of a new element. Benzol contains carbon and hydrogen, while alcohol contains carbon, hydrogen, and oxygen. Thus, not only does the idea of *multitude* come into play in absorption and radiation—that of *complexity* must also be taken into account. The speaker directed the particular attention of chemists to the molecule of water; the deportment of this substance towards radiant heat being perfectly anomalous, if the chemical formula at present ascribed to it be correct.

Sir William Herschel made the important discovery that, beyond the limits of the red end of the solar spectrum, rays of high heating power exist which are incompetent to excite vision. The speaker has examined the deportment of those rays towards certain bodies which are perfectly opaque to light. Dissolving iodine in the bisulphide of carbon, he obtained a solution which entirely intercepted the light of the most brilliant flames, while to the extra-red rays of the spectrum the same iodine was found to be perfectly diathermic. The transparent bisulphide, which is highly pervious to the heat here employed, exercised the same absorption as the opaque solution. A hollow prism filled with the opaque liquid was placed in the path of the beam from an

electric lamp, the light-spectrum was completely intercepted, but the heat-spectrum was received upon a screen and could be there examined. Falling upon a thermo-electric pile, its presence was shown by the prompt deflection of even a coarse galvanometer.

What, then, is the physical meaning of opacity and transparency as regards light and radiant heat? The luminous rays of the spectrum differ from the non-luminous ones simply in *period*. The sensation of light is excited by waves of ether shorter and more quickly recurrent than those which fall beyond the extreme red. But why should iodine stop the former and allow the latter to pass? The answer to this question no doubt is that the intercepted waves are those whose periods of recurrence coincide with the periods of oscillation possible to the atoms of the dissolved iodine. The elastic forces which separated these atoms are such as to compel them to vibrate in definite periods, and, when these periods synchronize with those of the ethereal waves, the latter are absorbed. Briefly defined, then, transparency in liquids as well as in gases is synonymous with *discord*, while opacity is synonymous with *accord* between the periods of the waves of ether and those of the molecules of the body on which they impinge. All ordinary transparent and colourless substances owe their transparency to the discord which exists between the oscillating periods of their molecules and those of the waves of the whole visible spectrum. The general discord of the vibrating periods of the molecules of *compound bodies* with the light-giving waves of the spectrum may be inferred from the prevalence of the property of transparency in compounds, while their greater harmony with the extra-red periods is to be inferred from their opacity to the extra-red rays. Water illustrates this transparency and opacity in the most striking manner. It is highly transparent to the luminous rays, which demonstrates the incompetency of its molecules to oscillate in the periods which excite vision. It is as highly opaque to the extra-red undulations, which proves the synchronism of its periods with those of the longer waves.

If, then, to the radiation from any source water shows itself to be eminently or perfectly opaque, it is a proof that the molecules whence the radiation emanates must oscillate in what may be called extra-red periods. Let us apply this test to the radiation from a flame of hydrogen. This flame consists mainly of incandescent aqueous vapour, the temperature of which, as calculated by Bunsen, is $3,259^{\circ}\text{C}$., so that, if transmission augment with temperature, we may expect the radiation from this flame to be copiously transmitted by the water. While, however, a layer of the bisulphide of carbon 0.07 of an inch in thickness transmits 72 per cent. of the incident radiation, and while every other liquid examined transmits more or less of the heat, a layer of water of the above thickness is entirely opaque to the radiation from the flame. Thus we establish *accord* between the periods of the molecules of cold water and those of aqueous vapour at a temperature of $3,259^{\circ}\text{C}$. But the periods of water have already been proved to be extra-red—hence those of the hydrogen flame must be extra-red also.

The absorption by *dry* air of the heat emitted by a platinum spiral raised to incandescence by electricity was found to be insensible, while that by the ordinary *undried* air was 6 per cent. Substituting for the platinum spiral a hydrogen flame, the absorption by dry air still remained insensible, while that of the undried air *rose to 20 per cent. of the entire radiation*. The temperature of the hydrogen flame was, as stated, $3,259^{\circ}$ C. ; that of the aqueous vapour of the air was 20° C. Suppose, then, the temperature of aqueous vapour to rise from 20° C. to $3,259^{\circ}$ C. , we must conclude that the augmentation of temperature is applied to *an increase of amplitude*, and not to the introduction of periods of quicker recurrence into the radiation.

The part played by aqueous vapour in the economy of nature is far more wonderful than hitherto supposed. To nourish the vegetation of the earth the actinic and luminous rays of the sun must penetrate our atmosphere ; and to such rays aqueous vapour is eminently transparent. The violet and the extra-violet rays pass through it with freedom. To protect vegetation from destructive chills the terrestrial rays must be checked in their transit towards stellar space ; and this is accomplished by the aqueous vapour diffused through the air. This substance is the great moderator of the earth's temperature, bringing its extremes into proximity, and obviating contrasts between day and night which would render life insupportable. But we can advance beyond this general statement, now that we know the radiation from aqueous vapour is intercepted, in a special degree, by water, and, reciprocally, the radiation from water by aqueous vapour : for it follows from this that the very act of nocturnal refrigeration which produces the condensation of aqueous vapour upon the surface of the earth—giving, as it were, a varnish of water to that surface—imparts to terrestrial radiation that particular character which disqualifies it from passing through the earth's atmosphere and losing itself in space.

And here we come to a question in molecular physics which at the present moment occupies the attention of able and distinguished men. By allowing the violet and extra-violet rays of the spectrum to fall upon sulphate of quinine and other substances Professor Stokes has changed the periods of those rays. Attempts have been made to produce a similar result at the other end of the spectrum—to convert the extra-red periods into periods competent to excite vision—but hitherto without success. Such a change of period, the speaker agreed with Dr. Akin in believing, occurs when a platinum wire is heated to whiteness by a hydrogen flame. In this common experiment there is an actual breaking up of long periods into short ones—a true rendering of universal periods visual. The change of refrangibility here effected differs from that of Professor Stokes : firstly, by its being in the opposite direction—that is, from lower to higher ; and, secondly, in the circumstance that the platinum is heated by the collision of the molecules of aqueous vapour, and before their heat has assumed the *radiant form*. But it cannot be doubted that the same effect would be produced by radiant heat of the same periods, provided the motion of the ether

could be rendered sufficiently intense. The effect in principle is the same, whether we consider the platinum wire to be struck by a particle of aqueous vapour oscillating at a certain rate, or by a particle of ether oscillating at the same rate.

By plunging a platinum wire into a hydrogen flame we cause it to glow, and thus introduce shorter periods into the radiation. These, as already stated, are in discord with water; hence we should infer that the transmission through water will be more copious when the wire is in the flame than when it is absent. Experiment proves this conclusion to be true. Water from being opaque opens a passage to 6 per cent. of the radiation from the flame and spiral. A thin plate of colourless glass, moreover, transmitted 58 per cent. of the radiation from the hydrogen flame; but when the flame and spiral were employed, 78 per cent. of the heat was transmitted. For an alcohol flame Knoblauch and Melloni found glass to be less transparent than for the same flame with a platinum spiral immersed in it; but Melloni afterwards showed that the result was not general—that black glass and black mica were decidedly more diathermic to the radiation from the pure alcohol flame. The reason for this is now obvious. Black mica and black glass owe their blackness to the carbon diffused through them. This carbon, as proved by Melloni, is in some measure transparent to the extra-red rays, and the speaker had succeeded in transmitting between 40 and 50 per cent. of the radiation from a hydrogen flame through a layer of carbon sufficient to intercept the light of the most brilliant flames. The products of combustion of the alcohol flame are carbonic acid and aqueous vapour, the heat of which is almost wholly extra-red. For this radiation, then, the carbon is in a considerable degree transparent, while for the radiation from the platinum spiral, it is in a great measure opaque. By the introduction of the platinum wire, therefore, the transparency of the pure glass and the opacity of its carbon were simultaneously augmented; but the augmentation of opacity exceeded that of transparency, and a difference in favour of opacity remained.

No more striking or instructive illustration of the influence of coincidence could be adduced than that furnished by the radiation from a carbonic oxide flame. Here the product of combustion is carbonic acid; and on the radiation from this flame even the ordinary carbonic acid of the atmosphere exerts a powerful effect. A quantity of the gas, only one-thirtieth of an atmosphere in density, contained in a polished brass tube four feet long, intercepted fifty per cent. of the radiation from the carbonic oxide flame. For the heat emitted by solid sources olefiant gas is an incomparably more powerful absorber than carbonic acid; in fact, for such heat the latter substance, with one exception, is the most feeble absorber to be found among the compound gases. For the radiation from the hydrogen flame, moreover, olefiant gas possesses twice the absorbent power of carbonic acid, but for the radiation from the carbonic oxide flame, at a common tension of one inch of mercury, while carbonic acid absorbs fifty per cent., olefiant gas absorbs only

twenty-four. Thus we establish the coincidence of period between carbonic acid at a temperature of 20° C. and carbonic acid at a temperature over $3,000^{\circ}$ C., the periods of oscillation of both the incandescent and the cold gas belonging to the extra-red portion of the spectrum.

It will be seen from the foregoing remarks and experiments how impossible it is to examine the effect of temperature on the transmission of radiant heat if different sources of heat be employed. Throughout such an examination the same oscillating atoms ought to be retained. The heating of a platinum spiral by an electric current enables us to do this, while varying the temperature between the widest possible limits. Their comparative opacity to the extra-red rays shows the general accord of the oscillating periods of our series of vapours with those of the extra-red undulations. Hence, by gradually heating a platinum wire from darkness up to whiteness, we gradually augment the discord between it and the vapours, and must therefore augment the transparency of the latter. Experiment entirely confirms this conclusion. Formic ether, for example, absorbs 45 per cent. of the radiation from a platinum spiral heated to barely visible redness; 32 per cent. of the radiation from the same spiral at a red heat; 26 per cent. of the radiation from a white-hot spiral, and only 21 per cent. when the spiral is brought near its point of fusion. Remarkable cases of inversion as to transparency occurred in these experiments. For barely visible redness formic ether is more opaque than sulphuric; for a bright red heat both are equally transparent, while, for a white heat, and still more for a nearly fusing temperature, sulphuric ether is more opaque than formic. This result gives us a clear view of the relationship of the two substances to the luminiferous ether. As we introduce waves of shorter period the sulphuric augments most rapidly in opacity; that is to say, its accord with the shorter waves is greater than that of the formic. Hence we may infer that the molecules of formic ether oscillate, on the whole, more slowly than those of sulphuric ether.

When the source of heat was a Leslie's cube filled with boiling water and coated with lampblack, the opacity of formic ether in comparison with sulphuric was very decided. With this source also the position of chloroform as regards iodide of methyl was inverted. For a white-hot spiral, the absorption of chloroform vapour being 10 per cent., that of iodide of methyl is 16; with the blackened cube as source the absorption by chloroform is 22 per cent., while that by the iodide of methyl is only 19. This inversion is not the result of temperature merely; for when a platinum wire, heated to the temperature of boiling water, was employed as a source, the iodide remained the most powerful absorber. All the experiments hitherto made by the speaker go to prove that from heated lampblack an emission takes place which synchronizes in an especial manner with chloroform. For the cube at 100° C., coated with lampblack, the absorption of chloroform is more than three times that by bisulphide of carbon; for the radiation from the most luminous portion of a gas-flame the absorption by chloroform

is also considerably in excess of that by bisulphide of carbon ; while, for the flame of a Bunsen's burner, from which the incandescent carbon particles are removed by the free admixture of air, the absorption by bisulphide of carbon is nearly twice that by chloroform. The removal of the incandescent carbon particles more than doubled in this instance the relative transparency of the chloroform. Testing, moreover, the radiation from various parts of the same flame, it was found that for the blue base of the flame the bisulphide was the most opaque, while for all the other portions of the flame the chloroform was most opaque. For the radiation from a very small gas flame, consisting of a blue base and a small white top, the bisulphide was also most opaque, and its opacity very decidedly exceeded that of the chloroform when the flame of bisulphide of carbon was employed as a source. Comparing the radiation from a Leslie's cube coated with isinglass with that from a similar cube coated with lampblack, at the common temperature of 100° C., it was found that, out of eleven vapours, all but one absorbed the radiation from the isinglass most powerfully ; the single exception was chloroform. It may be remarked that, *whenever, through a change of source, the position of a vapour as an absorber of radiant heat was altered, the position of the liquid from which the vapour was derived was changed in the same manner.*

It is still a point of difference between eminent investigators whether radiant heat, up to a temperature of 100° C., is monochromatic or not. Some affirm this ; some deny it. A long series of experiments has enabled the speaker to state that probably no two substances at a temperature of 100° C. emit heat of the same quality. The heat emitted by isinglass, for example, is different from that emitted by lampblack, and the heat emitted by cloth, or paper, differs from both. It is also a subject of discussion whether rock-salt is equally diathermic to all kinds of calorific rays. The differences affirmed to exist by one investigator being ascribed by others to differences of incidence from the various sources employed. MM. de la Provostaye and Desains maintain the former view, Melloni and M. Knoblauch maintain the latter. The question was examined by the author without changing anything but the temperature of the source. Its size, distance, and surroundings remained the same, and the experiments proved that rock-salt shares, in some degree, the defect of all other substances ; it is *not* perfectly diathermic, and it is more opaque to the radiation from a barely visible spiral than to that from a white-hot one.

In regard to the relation of radiation to conduction. Defining radiation, internal as well as external, as the communication of motion from the vibrating molecules to the ether, the speaker arrives, by theoretic reasoning, at the conclusion that the best radiators ought to prove the worst conductors. A broad consideration of the subject shows at once the general harmony of the conclusion with observed facts. Organic substances are all excellent radiators ; they are also extremely bad conductors. The moment we pass from the metals to their compounds we pass from a series of good conductors to bad ones,

and from bad radiators to good ones. Water, among liquids, is probably the worst conductor ; it is the best radiator. Silver, among solids, is the best conductor ; it is the worst radiator. In the excellent researches of MM. de la Provostaye and Desains the author finds a striking illustration of what he regards as a natural law ; that those molecules which transfer the greatest amount of motion to the ether, or in other words, radiate most powerfully, are the least competent to communicate motion to each other, or in other words, to conduct with facility.

[J. T.]

Friday, April 29, 1864.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. Vice-President,
in the Chair.

PROFESSOR ALEXANDER W. WILLIAMSON, F.R.S.
PRESIDENT OF THE CHEMICAL SOCIETY, &c.

*On the Classification of the Elements in relation to their
Atomicities.*

THE speaker proposed to bring under the consideration of the members some of the chemical grounds for doubling the atomic weights of all the metals in Gerhardt's system of atomic weights, excepting the alkali metals, silver, gold, boron, and the metals of the nitrogen series. A change which has been proposed mainly on physical grounds by Carmizzaro, and which seems to be obtaining the approbation of more and more chemists.

It is now about twenty years since Gerhardt drew attention to the error of the molecular weights, or equivalent weights, as he called them, which represented water as consisting of one atom of oxygen and one of hydrogen, and proposed to double the atomic weights of oxygen and of carbon.

If Gerhardt had taken Berzelius's atomic weights and, while translating them into the hydrogen scale, had halved the atomic weights of the alkali metals and boron, he would have given us at once the system which we now adopt, saving the rectification of a few formulæ, such as that of silver and oxide of uranium, &c; whereas by merely doubling oxygen, sulphur, selenium, and carbon, in the then existing system of atomic weights in the hydrogen scale, he really introduced a system in which there are between 30 and 40 atomic weights to correct, in lieu of one which needed only five or six such corrections. It would be unreasonable to apply this fact in any degree to the disparagement of Gerhardt's work. It only shows how tortuous is the road which leads to truth.

The discussion of the question involves chiefly the consideration of the classification of the elements under the respective heads of chlorine and of oxygen.

The first tribe containing those elements of which an atom combines with one atom of hydrogen or chlorine, or with three or with five, &c., whilst the second tribe contains elements of which each

TABLES OF ATOMIC WEIGHTS.

1st Class of Elements, which only furnishes an even number of atoms to each Molecule:—

Fl = 19	H = 1	N = 14
Cl = 35.5	Li = 7	P = 31
Br = 80	Na = 23	As = 75
J = 127	K = 39	Sb = 122
	Rb = 85	Bi = 210
	Cs = 133	B = 11
	Tl = 203	Au = 196
	Ag = 108	

2nd Class of Elements, which sometimes furnishes an odd, sometimes an even number of atoms to one Molecule:—

O = 16	C = 12	G = 9
S = 32	Si = 28	Yt = 64
Se = 79.5	Ti = 50	Ce = 92
I = 129	Sn = 118	La = 92
Ca = 40	Mo = 96	Dy = 96
Sr = 87.5	V = 137	U = 120
Ba = 137	W = 184	Zr = 89.5
Pb = 207	Pt = 197	Ta = 138
Mg = 24	Tr = 197	Cb = 195
Zn = 65	Os = 199	Th = 238
Cd = 112	Ro = 104	
Hg = 200	Ru = 104	
Al = 27.5	Pd = 106.5	
Fe = 56		
Cr = 52.5		
Mn = 55		
Co = 58.5		
Ni = 58.5		
Cu = 63.5		

atom combines with two atoms of chlorine, or other monads, or with four, or six. &c. The speaker did not, however, recommend that the two great classes of elements be thus distinguished from one another, for our chief evidence of atomic weights is derived from the study of the molecular weights of compounds, and the molecule is the unit to which results must be referred.

The first class is best described as furnishing only an even number of atoms to each molecule, whereas the second class sometimes furnishes an even, sometimes an uneven number of atoms to one molecule. The process of classifying the elements has followed the very natural order of establishing a certain number of well-defined families, which were

subsequently connected together by erratic members, which occasionally left their usual place to go over to some neighbouring family. Chlorine, bromine, and iodine have long been acknowledged to constitute a natural family; and there are some, though hardly sufficient reasons for placing fluorine at its head. The three elements have the same vapour volume as hydrogen in the free state, and we accordingly represent their respective molecules as Cl^2 , Br^2 , I^2 , corresponding to $\text{H}^2 = 2$ vols. They form hydrides of similar composition, and analogous properties, and of the same vapour volume. Their compounds with most metals are analogous and have the same atomic heat and general crystalline form. Their corresponding oxygen acids also exhibit considerable analogy.

With organic radicals they form neutral ethers, like ClC^2H^2 , $\text{ClC}^2\text{H}^2\text{O}$, and no acid ethers. So that when a molecule of alcohol or of acetic acid is replaced by chlorine, two atoms of chlorine take the place of one atom of oxygen, and give rise to a molecule of chloride of ethyle and a molecule of hydrochloric acid. They replace hydrogen atom for atom, taking out one, two, or three atoms, &c., according to circumstances. Their hydrogen compounds are all monobasic acids; for if, in a given quantity of hydrochloric or hydrobromic or hydriodic acid, we replace part only of the hydrogen by potassium, we get at once a neutral salt mixed with the remaining acid, which is undecomposed, and never an acid salt of the alkalis. Fluorine in this respect exhibits an anomaly which tends to remove it from this family to a biatomic one. For the acid fluoride of potassium is a well-defined compound of considerable stability, of which the existence points to the atomic weight 38 for fluorine, and the formula H_2F for hydrofluoric acid. Hydrofluoric acid, moreover, combines with various metallic fluorides—such as fluoride of silicon and fluoride of boron; and there are double fluorides of aluminium. &c., with alkaline fluorides, both well known and easily formed.

Similar double salts are, however, formed by chlorine; for instance, terchloride of gold combines with a molecule of hydrochloric acid, or of an alkaline chloride. Tetrachloride of platinum combines with two molecules of hydrochloric acid or of chloride of potassium, &c.

It is not possible to reconcile the constitution of these and similar bodies with one another and with the simpler compounds of chlorine, by any theory representing it as polyatomic, and as holding together the metallic atoms in these salts in virtue of its polyatomic character. On the other hand, hydrochloric acid and metallic chlorides of opposite properties cannot be assumed to be incapable of uniting with one another, while it is well known that oxides of basyloous properties unite with those of chlorous properties. Hydrochloric acid unites with ammonia, and we do admit that the two molecules are bound together into one by a chemical force of combination, and not by any tetratomic character of the hydrogen; and HCl or KCl combines with SO^2 by a similar force.

Again: oxygen, sulphur, selenium, and tellurium are admitted to

be truly analogous elements, for the parallelism of oxygen salts, and sulphur salts, affords abundant proof of the analogy of oxygen and sulphur, and the molecular volume of sulphur and selenium is found by Deville to agree at high temperatures with that of oxygen.

The elements selenium and tellurium form acids analogous to sulphurous and sulphuric acids respectively. When combined with organic radicals they form compounds of the same molecular volume in the form of vapour; and when any of them, such as oxygen, replaces hydrogen in an organic body, it takes out two atoms of hydrogen at a time, replacing each couple by one atom of oxygen, as in the formation of acetic acid from alcohol.

When we partially decompose water by potassium we get hydrate of potash formed, which is a molecule of water, from which half the hydrogen is expelled and replaced by potassium, and a second atom of potassium is required to displace the remaining hydrogen.

If we compare any proto-chloride with a corresponding oxide, either of a metal or organic radical, we find that the molecule of the oxide contains twice as many atoms of the metal or radical as the chloride, and that one atom from the oxygen family is equivalent to two atoms from the chlorine family.

When oxygen in alcohol is replaced by sulphur, no breaking up into sulphide of ethyle and sulphide of hydrogen takes place, as when the oxygen is replaced by chlorine or bromine.

Among the best known compounds there are several of which one atom combines, like an atom of oxygen or of sulphur, with two atoms like hydrogen or chlorine. Thus carbonic oxide, sulphurous acid, and olefiant gas are capable of combining in the proportion of one atom of the radical with two atoms of chlorine, forming the compound COCl_2 phosgene, SO_2Cl_2 chloro-sulphuric acid, and $\text{C}_2\text{H}_4\text{Cl}_2$ Dutch liquid; and these molecules have the same vapour volume as steam OH^2 . But in the free state the radicals have a vapour volume double as great as the equivalent quantity of oxygen, the atoms CO , SO_2 , C_2H_4 being as bulky as O^2 , so that whereas the molecule of oxygen and of sulphur consists of two atoms, that of carbonic oxide consists of one atom only, so also the molecule of sulphurous acid and of olefiant gas.

Another family of very marked characteristics is that consisting of N, P, As, Sb, Bi, each member of which combines with three atoms of hydrogen or of ethyle (C_2H_5), forming basic compounds analogous to ammonia. Their analogy in chemical reactions is also well known, as each of them forms an oxide corresponding to nitrous acid, and another corresponding to nitric acid.

The sulphides of arsenic and antimony are notorious for their great resemblance, and that of arsenious and antimonious acid is scarcely less striking. It even extends to isomorphism of their corresponding salts.

The atomic heat of the four last terms of the series is also very nearly the same, whilst that of nitrogen (examined of course as a gas) is considerably less. Then the molecule of phosphorus and of arsenic consists of four atoms, whilst that of nitrogen consists only of two,

showing a variety of constitution, which is by no means to be wondered at, when we recollect that these elements are not uniformly triatomic, but sometimes monatomic, pentatomic, &c., so that the molecule of free nitrogen consists of two monatomic atoms, or two triatomic, whilst the molecule of phosphorus and of arsenic is formed on the ammonia type of one triatomic atom and three monatomic atoms.

Another family may, perhaps, be made up of carbon and silicon, both of which form volatile tetrachlorides, and are sometimes biatomic, sometimes tetratomic in their acids.

Among metals, lithium, sodium, potassium, and probably also the new metals, rubidium, cesium, and thallium, have many important points of resemblance which show them to be monatomic. They replace hydrogen atom for atom, and form with many bibasic acids both normal and acid salts. Their chlorides form with tetra-chloride of platinum analogous double salts, and their sulphates form, with sulphate of alumina, &c., those well-characterized salts called alums. They do not form basic salts (unless when triatomic, like thallium). They have nearly the same atomic heat.

Silver is remarkable for several of the properties which we have noticed in the alkali metals. It is eminently monatomic, and disinclined to form basic salts. Its atomic heat also shows it to be monatomic. It appears to form an alum, and its sulphate has a great resemblance of form with the anhydrous sulphate of soda.

Gold also must from its specific heat, and the constitution of its two chlorides be classed among the metals which are monatomic or triatomic. Boron is evidently triatomic in its best known compounds, as proved by the ter-chloride and ethylide.

Among metals with strongly basylous properties, Ca, Sr, Ba, Pb, are connected by very close analogies. The general resemblance of their sulphates and carbonates, and the isomorphism of most of them, are too well known to need mention.

But lead has been obtained in combination with Ethyle, and the compound $\text{Pb}(\text{C}^2\text{H}^3)^4$ which corresponds to binoxide of lead, in which the two atoms of oxygen are replaced by four atoms of ethyle, and the compound $\text{Pb}(\text{C}^2\text{H}^3)^3\text{Cl}$ proves beyond a doubt that the metal is there tetrabasic.

Again : lead is pre-eminent for its tendency to form basic salts even with purely monatomic chlorous elements and radicals. Thus ordinary nitrate of lead, when warmed in aqueous solution with ceruse, expels carbonic acid from that compound, and forms the well-known and crystallizable basic nitrate— $\text{Pb} \begin{Bmatrix} \text{N} & \text{O}^2 \\ \text{H} & \text{O} \end{Bmatrix}$. If this be represented upon

the water type, it is formed from two molecules of water, $\begin{matrix} \text{H} & \text{O} \\ \text{H} & \text{O} \\ \text{H} & \text{O} \\ \text{H} & \text{O} \end{matrix}$ two

atoms of hydrogen, one from each molecule being replaced by the biatomic atom lead, whilst one of the remaining atoms of hydrogen is

NO^{a}
 replaced by N O^{a} , thus Pb^{O}
 H^{O}

But if the binary theory be adopted, it must be represented as lead combined with the radical N O^{a} , and also with the radical H O , and the biatomic lead holds thus two atoms together, just as much as biatomic oxygen holds together ethyle and hydrogen in alcohol. If we mix our lead compound with sulphate of silver, and heat with water, we replace the one atom of lead in it by two atoms of silver, getting a mixture of nitrate of silver and brown hydrated oxide of silver just as the replacement of oxygen in alcohol by Cl^{a} forms chloride of ethyle + hydrochloric acid.

We are thus led to consider these metals as biatomic, and to represent their oxides by the old formulæ Ca O , Ba O , Pb O , whilst carbonates, sulphides, and sulphates have formulæ like Ca C O^{a} , Ca S O^{a} , Ca S O^{a} , their chlorides, nitrates, and phosphates have formulæ like Ca Cl^{a} , $\text{Ca (N O}^{\text{a}})^{\text{a}}$, $\text{Ca}^{\text{a}} (\text{P O}^{\text{a}})^{\text{a}}$. Nitrate of potash has thus a similar formula $(\text{N O}^{\text{a}} \text{ K})$ to arragonite $\text{C O}^{\text{a}} \text{ Ca}$, and their isomorphism is no longer surprising. The same remark applies to calc spar and nitrate of soda.

Another analogous group of metals is the triad, magnesium, zinc, and cadmium, all volatile and forming salts which greatly resemble one another, and in many cases isomorphous. The constitution and properties of Frankland's zinc ethyle leaves no doubt of the biatomic character of zinc, for the compound $\text{Zn (C}^{\text{a}} \text{H}^{\text{b}})^{\text{a}}$ has the same molecular volume as ether $\text{O (C}^{\text{a}} \text{H}^{\text{b}})^{\text{a}}$, and if the atom of zinc were taken out and replaced by one atom of oxygen, there would be no change of volume. Then half the ethyle in zinc ethyle is replaceable by iodine, just as half the ethyle in ether is replaceable by potassium.

The biatomic character of this family being thus established, we can extend the conclusion to the other metals which form magnesian oxides, so called from the striking analogy of constitution of several of their salts with the corresponding salt of magnesia. In this manner we are led to adopt for iron, manganese, nickel, cobalt, and copper atomic weights corresponding to biatomic characters. The subsulphide of copper is thus represented by the formula $\text{Cu}^{\text{a}} \text{S}$, which is sufficiently similar to that of sulphide of silver, $\text{Ag}^{\text{a}} \text{S}$, to remove our surprise at their isomorphism. There is, moreover, in the reactions of alumina, sesqui-oxide of iron, sesqui-oxide of chromium, and sesqui-oxide of manganese, much resemblance. All these are weak bases, and their sulphates form with sulphate of potash those most characteristic salts called alums. The three first are isomorphous in the uncombined state, so that the conclusion established for iron and manganese may be extended to aluminium and chromium. But it is also arrived at by other means, for chromium in combination with oxygen and chlorine forms the well-characterized compound $\text{Cr O}^{\text{a}} \text{Cl}^{\text{a}}$ chloro-chromic acid, which contains the same quantity of oxygen and of chlorine as chloro-

sulphuric acid in two volumes of vapour, having 52.5 of chromium in the place of the 32 of sulphur of that compound. Again, chromic and sulphuric acids exhibit a marked resemblance of properties, the former being, if anything, even more distinctly bibasic than the latter, and their normal potash salts are isomorphous, so that chromium is abundantly proved to be similar to sulphur in atomicity, and brings in evidence of its own in favour of the biatomic character of aluminium, iron, and manganese. In like manner manganese in manganic acid is connected with sulphur in sulphuric acid, and requires a corresponding biatomic weight. The isomorphism and general analogy of permanganate of potash with perchlorate of potash has often been alluded to as pointing to the necessity of representing the former by a formula containing one large atom of manganese, $\text{Mn}(\text{O}^\circ \text{K})$: but although this formula, by assimilating the expressions for these two similar bodies, removes one difficulty, it creates at the same time another difficulty, by presenting a formula containing only one atom from the first family of elements. The speaker said he would not at present hazard any opinion regarding the propriety of removing this difficulty by doubling the above formulæ, together with that of perchlorate of potash, although he might remark that the constitution of the basic per-iodate of soda points to the formula $\text{I}^2(\text{O}^\circ \text{Na})_3(\text{H}^\circ \text{O})$.

An exceedingly strong ground for admitting for many heavy metals the atomic weight corresponding to a biatomic character was brought forward some time ago by Wurtz, who pointed out that adopting for oxygen the atomic weight 16, we get a half-molecule of water $\frac{\text{H}^2 \text{O}}{2}$ in one molecule of various salts if we consider the heavy metals monatomic.

Other metals are susceptible of reduction by similar analogies to the class of elements which are biatomic or tetratomic, &c. Thus mercury is proved by the ethylide and methylide to be biatomic by the fact that the compound for one atom of mercury with two atoms of ethyle or of methyle, occupies the same volume in the state of vapour as the compound of one atom of oxygen with two of ethyle or of methyle $\text{Hg}(\text{C}^\circ \text{H}^3)^2 = 2$ vols., and we can take out one atom of methyle from the bi-methylide of mercury, and replace it by an atom of chlorine, bromine, or iodine without disturbing the type, $\text{C}^\circ \text{H}^3 \text{Hg}$. The common bi-chloride of mercury has, moreover, a vapour volume corresponding to the biatomic character of the metal, and the same thing holds good of the vapour of metallic mercury itself, which has the same volume as the metal cadmium, and probably zinc, and the well-known biatomic radicals CO , SO^2 , $\text{C}^2 \text{H}^4$, but double the volume of the elements oxygen and sulphur. In the present state of our knowledge the speaker was not aware of any sufficient grounds for deciding which of these two constitutions of the free molecule of a biatomic element or radical is to be considered as normal and which is abnormal. On the one hand, mercury, cadmium, and all known

biatomic radicals have a molecule containing one atom, while the molecule of oxygen contains two atoms, and that of sulphur two at high temperatures and six at lower temperatures. Selenium is at high temperatures like sulphur. It has been amply shown by Dr. Odling and others that tin is biatomic and tetratomic in its two chlorides, and its compounds with the organic radicals and chlorine, &c., leave no room for doubt on the point.

By similar chains of evidence the remaining metals can be shown to belong to the great biatomic class containing already so many.

The vapour densities of the so-called sesqui chlorides of iron, aluminium, and chromium, as determined by Deville, show that the molecule of each of these bodies contains two atoms of metal and six atoms of chlorine, in fact the same quantity of metal as the molecule of the sesqui-oxide: this fact has been held to be an anomaly from the point of view adopted regarding their atomic weights. The speaker believed, however, that so far from being anomalous, these vapour densities are the least which can be reconciled with the conclusion that the metals permanently combine with even numbers of atoms from the first family, for if one atom of iron could on occasion combine with three atoms of chlorine to form one molecule, the law respecting it would assume the not very wise form,—that iron combines with an even number of atoms from the first family, except when it combines with an uneven number!

The fact is, that the sesqui-chlorides are not exceptions to the law, as at first sight they are suspected of being. Precisely the same remarks apply to the so-called subchloride of sulphur of which the molecule is S^2Cl^2 , as required by the law. So also cyanogen C^2N^2 , acetylene C^2H^2 , ethyle C^2H^{10} , &c. &c. Amongst exceptions, the speaker mentioned nitric oxide and calomel, both of which have vapour densities corresponding to the molecular formulæ NO and $HgCl$.

Many compounds are known to undergo decomposition on evaporation, and to be reproduced on condensation; thus NH^3O yields the two molecules NH^3 and H^2O , each with its own volume, as also SO^3H^2 yields SO^2 and H^2O . SO^4H^2 and PCl^3 are also known to yield on evaporation vapour corresponding to a breaking-up into two molecules; and there are strong reasons from analogy, as well as experimental evidence, to believe such decomposition. As, however, a high authority seems inclined to doubt the decomposition, the matter may be considered as still *sub judice*.

The existence of basic salts of mercury or copper, when apparently monatomic, is another class of apparent exceptions to the law. For if, in the sub-nitrate of mercury, the atom of metal really replaced one atom of hydrogen, just as potassium does in nitrate of potash, there ought not to be basic sub-nitrate of mercury, any more than a basic potash salt; whereas if the sub-nitrate of mercury contains, as the speaker asserted, in one molecule two atoms of metal and two atoms of the salt radical of the nitrates (NO^3), then a basic salt is as natural and intelligible a compound as the basic nitrate of the red oxide.

The action of ammonia on calomel confirms the molecular weight Hg^2Cl^2 ; for the compound $\text{N H}^2\text{Hg}^2\text{Cl}$, formed simultaneously with sal ammonia, proves that twice (Hg Cl) takes place in the reaction.

[A. W. W.]

Friday, May 6, 1864.

COLONEL PHILIP JAMES YORKE, F.R.S. in the Chair.

PROFESSOR ROSCOE, F.R.S.

On the Metal Indium and Recent Discoveries on Spectrum Analysis.

SINCE the spring of 1862, when the speaker delivered a course of three lectures in this Institution on the Spectrum Discoveries, much has been done to increase our knowledge of Spectrum Analysis, but the whole subject is still in its infancy, and the further we advance the more we find remains to be known.

No less than four new elementary bodies have already been discovered by means of Spectrum Analysis : Cæsium and Rubidium, by Bunsen ; Thallium, by Mr. Crookes ; and Indium, by Reich and Richter, of Freiberg ; whilst the foundations of Solar Chemistry, laid by Kirchhoff, have been rendered more secure by the observations of Cooke, in America ; Donati, in Italy ; and Miller and Huggins, in England.

Cæsium and rubidium were at first only found in one or two mineral waters ; they have since been shown to be widely distributed in the vegetable as well as in the mineral kingdom ; they have been obtained in considerable quantities from the beet-root salt, and found in the ashes of tea and coffee, thus proving that they occur commonly in soil ; whilst, quite recently, M. Pisani has found that a mineral, called pollux, occurring in Elba, contains 34 per cent. of cæsium, this metal having been mistaken for potash in the analyses which had previously been made of this substance. Thallium and its compounds have been obtained in large quantities, and their properties fully investigated by Crookes and Lamy, whilst this metal has not only been found in iron pyrites, but also in large quantities by Schrötter, in the mica of Zinnwald, and in lepidolite, from Moravia. Thallium has been shown by Boettger to occur together with cæsium and rubidium in the mineral water of Nauheim, near Frankfort ; Boettger has, moreover, shown that thallium is contained in the vegetable kingdom, he has found it in the yeast of the vinous fermentation ; so that thallium exists in wine ; also in treacle, tobacco, and chicory. If 4 lbs. of any of these substances are employed, a sufficient quantity of thallium can be obtained as the double platinum-chloride to enable its presence to be easily detected. Professor Bunsen has informed the speaker that he has found a mother liquor from the Hartz, which contains so much thallium, that the

iodide can be obtained by direct precipitation in quantity at the rate of 10s. per lb. The speaker exhibited the spectrum of the Nauheim salt, which contains the three new elements; the spectrum of each metal is well seen by placing the mixed platino-chlorides in the electric arc.

Drs. Reich and Richter, of Freiberg, in Saxony, have lately discovered a fourth new metal in the Freiberg zinc blende.* This metal has been termed Indium, from the two splendid indigo-blue lines which characterize its spectrum. Through the kindness of Professor Richter, the speaker had been placed in possession of a few grains of this new metal, the spectrum of which was exhibited by the electric lamp. In its chemical relations it resembles zinc, with which it is associated in nature; the metal can be reduced before the blowpipe to a malleable bead, when it forms a soft, ductile bead, which imparts streaks to paper on rubbing, and possesses a colour lighter than that of lead, being about the same as that of tin. The metallic bead dissolves in hydrochloric acid with the evolution of hydrogen. The oxide of indium is formed as a yellow fusible incrustation when the metal is heated before the blowpipe on charcoal. Indium differs from zinc in the insolubility of the hydrated oxide in excess of both ammonia and caustic potash. This new element may be separated from all the known metals by precipitating its sulphide in alkaline solution, and by throwing down the hydrated oxide first with ammonia and then with caustic potash; and, lastly, by precipitating the iron with dilute solution of bicarbonate of sodium. The hydrated oxide of indium then remains in solution in the pure state. Indium may be readily detected when present in its pure compounds by the deep purple tint which these impart to flame. The characteristic lines are, however, best seen when a small bead of indium salt is placed between two poles, from which an electric spark passes; the lines $\text{In } \alpha$ and $\text{In } \beta$ fall respectively upon divisions 107.5, and 140 of the photographic scale of the spectroscope, when $\text{Na} = 50$, and $\text{Sr } \delta = 100.5$. Up to the present time indium has been only found in the very smallest quantity, and hence the atomic weight of the metal and the composition of its salts have not yet been determined; in fact, the speaker was led to infer that Professor Richter sent him nearly all the compound of the metal remaining from the investigation of its properties, for the purpose of illustrating this discourse. It has only as yet been detected in the zinc blende of Freiberg; but it will, doubtless, soon be discovered in larger quantities, and its compounds more closely studied.

As regards the spectra of the well-known metals, our knowledge has been much increased by the publication of the second series of Kirchhoff's maps of the solar spectrum and the spectra of the chemical elements (Macmillan and Co.). In these Kirchhoff has marked the position of the bright lines of no less than thirty metals, and indicated those which, as they coincide with a dark solar line, reveal the presence of the par-

* Phil. Mag. for March, 1864. Series 4, vol. xxvii. p. 199.

ticular metal in the sun's atmosphere. Kirchhoff's maps now embrace the whole of the visible spectrum from the line A in the extreme red, to the line G in the indigo; beyond these limits the intensity of the light passing through his three prisms became too slight to enable him to draw the lines. The observations thus made of coincidences of metallic with solar lines in the red and indigo portions of the spectrum, confirm the conclusions drawn by Kirchhoff from his earlier observations, with the exception of the presence of potassium. This metal is not seen in the solar atmosphere, the potassium red line is not coincident with the solar line A, as it was supposed to be, nor with any other dark solar line. No metal, in addition to those previously observed, was found to possess lines coincident with solar lines, and hence the number of bodies known to be present in the sun has not been increased.

The experiments of Mr. Huggins on the spectra of the metallic elements, made with an instrument of six prisms, although not yet published in full, promise to add greatly to our knowledge on this subject; one interesting observation may be cited; *viz.* that the spectrum of sodium has been found to contain three pairs of lines in addition to those corresponding to the dark double line D, and that these also coincide with dark solar lines, adding to the evidence previously possessed of the existence of sodium in the sun. The audience had been already made acquainted with Dr. Miller's important researches on the photographic spectra of the metals, and with the valuable observations made by himself and Mr. Huggins on the spectra of the fixed stars. Connected with this part of the subject may be mentioned Professor Stokes's interesting investigation on the long spectrum of the electric spark, in which he shows that the vapour of certain metals, such as iron and magnesium, when heated by the passage of an electric spark, emit rays of so high a degree of refrangibility, that they are situated at a distance from the lines H, ten times as great as that of the whole visible spectrum from A to H. These highly refrangible rays only become visible at the highest temperatures, and they are not seen in the solar spectrum, although the less refrangible iron and magnesium lines are present. Hence it has been suggested that the temperature of the sun must be lower than that of the electric spark in which these lines are developed. This conclusion appears legitimate only if we know that these rays of high refrangibility are not absorbed in passing through our atmosphere; and an investigation of great interest here presents itself for those who ascend into the higher regions of the atmosphere.

The observations of Dr. Robinson upon metallic spectra have led this astronomer to doubt the validity of some of the conclusions arrived at by Kirchhoff concerning the existence of a separate and non-coincident set of lines in the spectrum of each metal. It seems, however, that Dr. Robinson employed only one prism and a low magnifying power, so that we must conclude that the observations from which he deduces the coincidence of certain lines as proving their identity in several

metals, cannot impugn the results obtained by help of a larger instrument of sufficient power to resolve these apparent coincidences.

The original statement made by Bunsen and Kirchhoff concerning the spectra of the metals still remains unopposed by a single well-established fact—the statement, namely, that when a metal is heated up to a certain point, the spectrum of its incandescent vapour contains a number of fine bright lines which do not change their position with increase of temperature, and are not coincident with the lines of any other known substance. There is, however, no doubt of the fact that in the spectra of certain metals or metallic compounds new lines are developed by increase of temperature; and also that certain metals, as calcium, barium, and strontium, yield spectra of two kinds; one of these, seen at the lower temperature, and consisting of broad bands, being resolved at a higher temperature into bright lines. These bright lines do not undergo any further change on elevation of temperature, and characterize the true metallic spectrum, whilst the band-spectrum is probably produced by the incandescent vapour of a metallic compound which is decomposed at a higher temperature.

Our knowledge of the spectra of the non-metallic elements is, as yet, in a very incomplete state. To the researches of Plücker, we are especially indebted for information on this subject; he has shown that each metalloid possesses a peculiar and characteristic spectrum; hydrogen, for instance, yielding only three bright lines, all of which are coincident with dark solar lines; and nitrogen exhibiting a complicated series of bands. Plücker has lately come to the conclusion that many non-metallic elementary bodies, and among them sulphur and nitrogen, exhibit two distinctly different spectra when the temperature is altered, in this respect resembling the metals of the alkaline earths. This difference Plücker ascribes to the existence of these elements in two allotropic conditions.

A singular relation with regard to what have been termed the carbon lines was observed by the speaker. It has been stated that all the various forms of carbon compounds when in the state of incandescent gas, yield identical spectra. This proves not to be the case; the spectrum obtained from the flame of olefiant gas is different from that obtained by the electric discharge through a vacuum of the same gas; whilst the spark passing through a cyanogen vacuum produces a spectrum identical with that of the olefiant gas-flame, and through the carbonic oxide vacuum a spectrum coincident with that of the spark through olefiant gas vacuum.

As an illustration of the application of abstract scientific principles to useful practical purposes, the speaker stated that he had lately applied spectrum analysis to the manufacture of steel by the Bessemer process. One of the great drawbacks to the successful practical working of Mr. Bessemer's beautiful process for converting cast-iron directly into steel, has been the difficulty of determining the exact point at which the blast of air passing through the molten metal is to be stopped. The conversion of five tons of cast-iron into cast-

steel usually occupies from fifteen to twenty minutes, according to the varying conditions of weather, quality of the iron, strength of the blast, &c. If the blast be continued for ten seconds after the proper point has been attained, or if it be discontinued ten seconds before that point is reached, the charge becomes either so viscid that it cannot be poured from the converting vessel into the moulds, or it contains so much carbon as to crumble under the hammer. Up to the present time, the manufacturer has judged of the condition of the metal by the general appearance of the flame which issues from the mouth of the converting vessel. Long experience enables the workman thus to detect, with more or less exactitude, the point at which the blast must be cut off. It appeared to the speaker that an examination of the spectrum of this flame might render it possible to determine this point with scientific accuracy, and that thus an insight might be gained into the somewhat complicated chemical changes which occur in this conversion of cast-iron into steel. At the request of Messrs. John Brown and Co., of the Atlas Works, Sheffield, the speaker investigated the subject, and succeeded in obtaining very satisfactory and interesting results. The instrument employed was an ordinary Steinheil's spectroscope, furnished with photographic scale and lamp, and provided with a convenient arrangement for directing the tube carrying the slit towards any wished-for part of the flame, and for clamping the whole instrument in the required position. By help of such an arrangement the spectrum of the flame can be most readily observed, and the changes which periodically occur can be most accurately noted.

The light which is given off by the flame in this process is most intense; indeed, a more magnificent example of combustion in oxygen cannot be imagined—and a cursory examination of the flame spectrum in its various phases reveals complicated masses of dark absorption bands and bright lines, showing that a variety of substances are present in the flame in the state of incandescent gas. By a simultaneous comparison of these lines in the flame-spectrum with the well-known spectra of certain elementary bodies, the speaker has succeeded in detecting the presence of the following substances in the Bessemer flame:—Sodium, potassium, lithium, iron, carbon, phosphorus, hydrogen, and nitrogen.

A further investigation with an instrument of higher dispersive and magnifying powers than that employed, will doubtless add to the above list; and an accurate and prolonged study of this spectrum will probably yield very important information respecting the nature of the reactions occurring within the vessel. Already the investigation is so far advanced that the point in the condition of the metal at which it has been found necessary to stop the blast can be ascertained with precision; and thus, by the application of the principles of Spectrum Analysis, that which previously depended on the quickness of vision of a skilled eye has become a matter of exact scientific observation.

Another interesting practical application of our knowledge concerning the properties of the kind of light which certain bodies emit when

heated, is the employment of the light evolved by burning Magnesium wire for photographic purposes. The spectrum of this light is exceedingly rich in violet and ultra-violet rays, due partly to the incandescent vapour of magnesium, and partly to the intensely-heated magnesia formed by the combustion. Professor Bunsen and the speaker in 1859 determined the chemically active power possessed by this light, and compared it with that of the sun; and they suggested the application of this light for the purpose of photography. They showed* that a burning surface of magnesium wire, which, seen from a point at the sea's level, has an apparent magnitude equal to that of the sun, effects on that point the same chemical action as the sun would do if shining from a cloudless sky at a height of $9^{\circ} 53'$ above the horizon. On comparing the *visible* brightness of these two sources of light, it was found that the brightness of the sun's disc, as measured by the eye, is 524.7 times as great as that of burning magnesium wire when the sun's zenith distance is $67^{\circ} 22'$; whilst at the same zenith distance the sun's *chemical* brightness is only 36.6 times as great. Hence the value of this light as a source of the chemically active rays for photographic purposes becomes at once apparent.

Professor Bunsen and the speaker state in the memoir above referred to, that, "the steady and equable light evolved by magnesium wire, burning in the air, and the immense chemical action thus produced; render this source of light valuable as a simple means of obtaining a given amount of chemical illumination, and that the combustion of this metal constitutes so definite and simple a source of light for the purpose of photo-chemical measurement, that the wide distribution of magnesium becomes desirable. The application of this metal as a source of light may even become of technical importance. A burning magnesium wire of the thickness of 0.297 millimetre, evolves, according to the measurement we have made, as much light as 74 stearine candles of which five go to the pound. If this light lasted one minute, 0.987 metre of wire, weighing 0.120 grammes, would be burnt. In order to produce a light equal to 74 candles burning for ten hours, whereby about 20 lbs. of stearine is consumed, 72.2 grammes ($2\frac{1}{2}$ ounces) of magnesium would be required. The magnesium wire can be easily prepared by forcing out the metal from a heated steel press having a fine opening at bottom; this wire might be rolled up in coils on a spindle, which could be made to revolve by clockwork, and thus the end of the wire, guided by passing through a groove or between rollers, could be continually pushed forward into a gas or spirit lamp flame in which it would burn."

It afforded the speaker great pleasure to state that the foregoing suggestion had now been actually carried out. Mr. Edward Sonstadt has succeeded in preparing magnesium on the large scale, and great credit is due to this gentleman for the able manner in which he has

* Phil. Trans. 1859, p. 920.

brought the difficult subject of the metallurgy of magnesium to its present very satisfactory position.

Some fine specimens of crude and distilled magnesium weighing 3 lbs. were exhibited as manufactured by Mr. Sonstadt's process, by Messrs. Mellor & Co. of Manchester.

The wire is now to be had at the comparatively low rate of 3*d.* per foot; * and half-an-inch of the wire evolves on burning light enough to transfer a positive image to a dry collodion plate; whilst by the combustion of 10 grains a perfect photographic portrait may be taken, so that the speaker believed that for photographic purposes alone the magnesium light will prove most important. The photochemical power of the light was illustrated by taking a portrait during the discourse. In doing this the speaker was aided by Mr. Brothers, photographer, of Manchester, who was the first to use the light for portraiture.

[H. E. R.]

Friday, June 3, 1864.

SIR HENRY HOLLAND, Bart. M.D. F.R.S. Vice-President, in the Chair.

EDWARD FRANKLAND, Esq. F.R.S.

PROFESSOR OF CHEMISTRY, ROYAL INSTITUTION.

On Recent Chemical Researches in the Royal Institution.

AMONGST the branches of inquiry that have engaged the attention of chemists during the past fifteen years, there can scarcely be two opinions as to the paramount importance of those investigations, which have had for their object the discovery of the internal structure of chemical compounds, and especially of organic compounds; for it is by thus studying the architecture of these bodies, that we become acquainted with the plans according to which nature herself constructs them under the influence of what we term vitality, and that we are enabled to imitate her operations. The vast number of organic compounds that can now be produced, without the aid of life in any form, some of them even constituting a part of the food of man, affords ample testimony to the importance of this field and the success with which it has been cultivated.

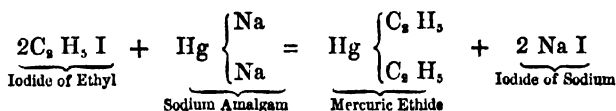
The ultimate analytical composition of a chemical compound affords us little or no information available for the production of that compound, artificially; but the moment the internal arrangement of the atoms becomes known, the constructive process at once suggests itself. Such a problem may be attacked in two distinct ways, either by taking the compound to pieces, or by building it up from its proximate constituents. More than twelve years ago, the speaker had applied the latter or synthetical process to the investigation of organic compounds containing metals, some of the results of which he had communicated to the members on a previous occasion. A like scrutiny must be applied to other families of organic substances if we are to become equally acquainted with their molecular construction. It was the application of the synthetical process to an important family of organic substances, that had formed the basis of the investigations recently carried on in the chemical laboratory of the Institution. In the execution of this work the speaker had been enthusiastically joined by his

friend, Mr. Duppa, who had in an eminent degree contributed to whatever success had attended their labours.

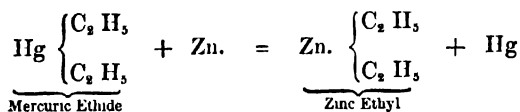
The family of organic acids thus attacked, and which is represented by lactic acid, had for some years past excited the interest and attention of chemists, but although much laborious investigation had been expended upon it, especially by Kolbe and Wurtz, yet the constitution of these acids was still far from being established. Like any effort to overcome a difficulty, such an investigation required the selection of a plan of attack, and the preparation of the agents, or weapons, by which the assault was to be made. The speaker had already proved in a paper communicated to the Royal Society, that oxalic acid was the basis or model of the family of acids to be investigated. This fact showed the path by which the subject was to be approached, and he then went on to describe the principles according to which the weapons were constructed.

In Mechanics, the engineer proportions the force which he employs to the effect required to be produced, and it was considered one of the greatest achievements in such control of mechanical force, when Mr. Nasmyth's steam-hammer could be made, at one moment to deliver a blow gentle enough to break the shell of a nut without crushing the kernel, and at the next to descend with a force sufficient to smash a block of granite and shake the ground beneath it. As in mechanics, where we deal with masses, so in chemistry, where we have to do with atoms, it is also necessary to apply a properly graduated amount of force, and to apply it in the right direction. Chemistry was yet far behind mechanics in this faculty of graduating force, but by availing ourselves of certain chemical reactions, we had the power, as it were, of gradually storing up force in the atoms of bodies, and of delivering the blow when the force had become strong enough to effect the change required. In this way the comparatively inert radicles or molecules, methyl, ethyl, amyl, &c., could be invested with chemical energy sufficient to force their entrance into oxalic acid. The process of thus endowing these radicles with force was likened to the gradual winding up of a weight to the height necessary for the production of a given effect by its subsequent fall. For this purpose a force external to the atoms to be elevated was obviously required. The first supply of this force was taken from sodium; but sodium, although competent to raise the molecules of ethyl or methyl to a great elevation, was yet too rough in the use of its power, for if we attempted, by its sole agency, to elevate these molecules, they were actually torn to pieces by the violence of the effort. The action of the sodium must, therefore, be moderated by combining it with mercury; much of its power was thus lost, but sufficient still remained for the purpose, if rightly employed.

This sodium amalgam on being brought into contact with the iodides of methyl, ethyl, or amyl refused to exert any action, but on the addition of a few drops of acetic ether, which acted in this case like a ferment, the sodium separated the iodine from the ethyl, whilst the latter united itself with the mercury.



By this association with mercury, the energy of the ethyl was greatly increased, but it still lacked sufficient power for its attack upon oxalic acid; having once commenced its ascent, however, the further elevation of the ethyl became comparatively easy. It was only necessary to digest the mercuric ethide, procured as above described, with metallic zinc, at a temperature of 100° C. for several hours, in order to replace the mercury with zinc, by which means zinc ethyl was obtained.



The zinc ethyl thus obtained possessed far greater energy than the mercuric ethide from which it was derived,* and, in fact, in this compound, the ethyl became fully armed for the contemplated expedition. The speaker, however, showed that its power could be still further increased by the addition of the metal lithium. By these processes the following chemical compounds and weapons of attack had been manufactured :--

NAME.	Formula.
Mercuric Methide	$\text{Hg} \begin{Bmatrix} \text{CH}_3 \\ \text{CH}_3 \end{Bmatrix}$
Mercuric Iodo-methide	$\text{Hg} \begin{Bmatrix} \text{CHI}_3 \\ \text{I} \end{Bmatrix}$
Mercuric Ethide	$\text{Hg} \begin{Bmatrix} \text{C}_2\text{H}_5 \\ \text{C}_2\text{H}_5 \end{Bmatrix}$
Mercuric Iodo-ethide	$\text{Hg} \begin{Bmatrix} \text{C}_2\text{H}_5 \\ \text{I} \end{Bmatrix}$

* The intense chemical energy of zinc ethyl was shown experimentally by a fountain of the liquid, which played perpendicularly to the height of 6 or 8 feet, forming a fiery jet of blue and white flame.

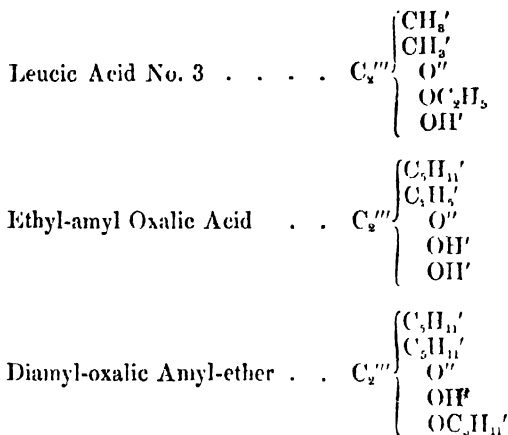
Mercuric Chlor-ethide . . .	Hg	$\left\{ \begin{array}{l} C_2 H, \\ Cl \end{array} \right.$
Mercuric Amylide.	Hg	$\left\{ \begin{array}{l} C, H_{11} \\ C_3 H_{11} \end{array} \right.$
Mercuric Iod-amylide. . . .	Hg	$\left\{ \begin{array}{l} C_3 H_{11} \\ I \end{array} \right.$
Mercuric Chlor-amylide . . .	Hg	$\left\{ \begin{array}{l} C_3 H_{11} \\ Cl \end{array} \right.$
Zincmethide.	Zn	$\left\{ \begin{array}{l} CH_4 \\ CH_3 \end{array} \right.$
Zincethide	Zn	$\left\{ \begin{array}{l} C_2 H_5 \\ C_2 H_5 \end{array} \right.$
Zincamylide.	Zn	$\left\{ \begin{array}{l} C_3 H_{11} \\ C_3 H_{11} \end{array} \right.$
Lithio-mercuric Methide . . .	$\begin{array}{c} Hg'' \\ Li' \end{array}$	$\left\{ \begin{array}{l} CH_3' \\ CH_4' \\ CH_3' \end{array} \right. ?$
Lithio-mercuric Ethide . . .	$\begin{array}{c} Hg'' \\ Li' \end{array}$	$\left\{ \begin{array}{l} C_2 H_5' \\ C_3 H_5' \\ C_2 H_5' \end{array} \right. ?$
Lithio-zinc Methide	$\begin{array}{c} Zn'' \\ Li' \end{array}$	$\left\{ \begin{array}{l} CH_3' \\ CH_3' \\ CH_3' \end{array} \right. ?$
Lithio-zinc Ethide.	$\begin{array}{c} Zn'' \\ Li' \end{array}$	$\left\{ \begin{array}{l} C_2 H_5' \\ C_2 H_5' \\ C_2 H_5' \end{array} \right. ?$

The speaker then described the action of several of these bodies upon oxalic acid, or rather oxalic ether. This action consisted in the removal of one atom of oxygen from oxalic acid, and its substitution by two atoms of ethyl, methyl, &c. Thus, by the action of zincethyl, oxalic acid was transformed into leucic acid—a body that had previously been obtained from animal tissues, especially from the spleen and lungs. By acting upon oxalic ether with the zinc compounds of other organic radicles, a large number of acids belonging to the lactic series, and hitherto unknown, could be produced. Many of these acids were isomeric with each other, that is, possessed the same percentage composition, but differed in their interior architecture. Thus leucic acid was susceptible of no less than nine isomeric modifications, three of which had already been obtained by the method now described. The following table shows the internal structure of these isomeric leucic acids :—

$C_3''' \left\{ \begin{array}{l} C_2H_3' \\ C_2H_3' \\ O'' \\ OH' \\ OH' \end{array} \right.$	$C_3''' \left\{ \begin{array}{l} C_2H_3' \\ H' \\ O'' \\ OC_2H_3' \\ OH' \end{array} \right.$	$C_3''' \left\{ \begin{array}{l} C_2H_3' \\ CH_3' \\ O'' \\ OCH_3' \\ OH' \end{array} \right.$
$C_3''' \left\{ \begin{array}{l} CH_3' \\ CH_3' \\ O'' \\ OC_2H_3' \\ OH' \end{array} \right.$	$C_3''' \left\{ \begin{array}{l} C_2H_7' \\ CH_3' \\ O'' \\ OH' \\ OH' \end{array} \right.$	$C_3''' \left\{ \begin{array}{l} C_2H_7' \\ H' \\ O'' \\ OCH_3' \\ OH' \end{array} \right.$
$C_3''' \left\{ \begin{array}{l} CH_3' \\ H' \\ O'' \\ OC_4H_7' \\ OH' \end{array} \right.$	$C_3''' \left\{ \begin{array}{l} C_4H_3' \\ H' \\ O'' \\ OH' \\ OH' \end{array} \right.$	$C_3''' \left\{ \begin{array}{l} H' \\ H' \\ O'' \\ OC_4H_9' \\ OH' \end{array} \right.$

The following is a list of acids which, with their compounds, have thus been produced and investigated during the past year in the laboratory of the Institution.

NAME.	Formula.
Dimethyloxalic Acid No. 1	$C_3''' \left\{ \begin{array}{l} CH_3' \\ CH_3' \\ O'' \\ OH' \\ OH' \end{array} \right.$
Dimethyloxalic Acid No. 2	$C_3''' \left\{ \begin{array}{l} CH_3' \\ H' \\ O'' \\ OCH_3' \\ OH' \end{array} \right.$
Leucic Acid No. 1	$C_3''' \left\{ \begin{array}{l} C_2H_3' \\ C_2H_3' \\ O'' \\ OH' \\ OH' \end{array} \right.$
Leucic Acid No. 2	$C_3''' \left\{ \begin{array}{l} C_2H_3' \\ CH_3' \\ O'' \\ OCH_3' \\ OH' \end{array} \right.$



These reactions proved that lactic acid, the representative of this family of acids, was also cast in the mould of oxalic acid. Thus, the latter deadly organic body was converted by the removal of one atom of oxygen and its substitution by one of hydrogen and one of methyl into the harmless acid of sour milk, a constituent of the juices of the human body, and an agent, no doubt, of importance in the transformations attending animal life. A similar marvellous transmutation of character is met with in the highly poisonous arsenic acid, which, by the exchange of one atom of oxygen for two of methyl, is converted into the innocuous, though perfectly soluble, cacodylic acid.

The speaker concluded as follows:—Here, then, we have a most prolific reaction, capable of furnishing an immense number of new organic bodies, and at the same time indicating to us the very simple manner in which nature evolves some of her, apparently, most complex results. By a species of progressive development, this simply organized oxalic acid becomes gradually elevated, cultivated, and transformed into bodies, which, when viewed by one ignorant of their true origin, appear to possess a hopeless complexity. Not only do we now gain a clear insight into the architecture of these acids, but we can take the very elements of which they are composed, and build them up unaided by any vital processes. We need not even go for oxalic acid, our very type or model, either to the wood-sorrel, or the lichen, which, by means of this acid, corrodes the rock upon which it grows; for we have the power both to lay the foundation and to build the superstructure of these organic bodies, without the least assistance from either animal or vegetable life. And is it too much to hope, that, by analogous inductive scrutiny, even the most obscure and complex physiological phenomena of life itself will one day yield to scientific research, and become to us as clear and simple as they are now dark and unintelligible? But to

accomplish this, the human intellect must prepare itself for efforts far more difficult than any it has yet made. Hitherto the more palpable and simple phenomena of nature have been the first to attract the attention of philosophers, whilst the more recondite and hidden, constituting increasingly difficult subjects of research, have been left for future explorers. Thus, although we are still scarcely advanced beyond the condition of children gathering pebbles on the shore of the boundless ocean of knowledge, yet those pebbles, never easy to find, are now no longer left dry on the beach. They must be dragged from the grip of the waves by patient and cunning toil. Difficulties innumerable and appalling confront us, but let the human intellect be only left free and untrammelled, and it will surely accomplish the task set before it.

[E. F.]

Friday, June 10, 1864.

WILLIAM ROBERT GROVE, Esq. M.A. Q.C. F.R.S. Vice-President,
in the Chair.

JOHN TYNDALL, Esq. F.R.S. M.R.I.
PROFESSOR OF NATURAL PHILOSOPHY, ROYAL INSTITUTION.

On a Magnetic Experiment.

THERE are two words which are very often employed in scientific writings—matter and force. The definition of each involves the conception of the other. We know nothing of force save through its operations upon matter, and we know nothing of matter save through the manifestations of its force. The characteristics of any force must be sought in the material changes which it is competent to produce. Some years ago I felt a great interest in the subject of magnetism, and in those years I devised an apparatus to enable me to investigate certain mechanical effects which accompany the act of magnetization. I wished to apply this apparatus to diamagnetic bodies as well as paramagnetic ones—to bodies such as bismuth, as well as to bodies such as iron. I intend this evening to show you the action of this instrument, and to give, if I can, some explanation of the experiments of others which have been confirmed by my own.

Let us pass quickly in review the excitation of this wonderful power of magnetism. Here is a strong horseshoe magnet set upright, and here is a bent bar of steel, whose arms are the same distance apart as those of the horseshoe magnet. I draw the bent steel bar over the ends, or the poles, as they are called, of the magnet. It suddenly obtains the power of attracting this iron keeper and holding it fast. I reverse the stroke of the steel bar: its virtue has now disappeared; it is no longer competent to attract the keeper. I continue the stroke of the steel bar in the last direction, and now it is again competent to attract the iron: thus I can at will magnetize and demagnetize this bent piece of steel.

Here is a fine permanent magnet constructed by Logeman of Haarlem, and competent to carry a great weight. Here, for example, is a dish of iron nails which it is able to empty. At the other side of the table you observe another mass of metal, bent like the Logeman magnet, but not, like it, naked. This mass, moreover, is not steel, but iron, and it is surrounded by coils of copper wire. It is intended to illustrate the excitement of magnetism by electricity. At the

present moment this huge bent bar is so inert as to be incapable of carrying a single grain of iron. I now send an electric current through the coils that surround it, and its power far transcends that of the steel magnet on the other side. It can carry fifty times the weight. It holds a 56lb. weight attached to each of its poles, and it empties this large tray of iron nails when they are brought sufficiently near it. I interrupt the current: the power vanishes, and the nails fall.

Now the magnetized iron cannot be in all respects the same as the unmagnetized iron. Some change must take place among the molecules of the iron bar at the moment of magnetization. And one curious action which accompanies the act of magnetization I will now try to make sensible to you. Other men laboured, and we are here entering into their labours: the effect I wish to make manifest was discovered by Mr. Joule,* and was subsequently examined by MM. De la Rive, Wertheim, Marian, Matteucci, and Wartmann. It is this. At the moment when the current passes through the coil surrounding the electro-magnet, a clink is heard emanating from the body of the iron, and at the moment the current ceases a clink is also heard. In fact, the acts of magnetization and demagnetization so stir the atoms of the magnetized body that they, in their turn, can stir the air and send sonorous impulses to our auditory nerves.

I have said that the sounds occur at the moment of magnetization, and at the moment when magnetization ceases; hence, if I can devise a means of making and breaking in quick succession the circuit through which the current flows, I can obtain an equally quick succession of sounds. I do this by means of a contact-breaker which belongs to a Ruhmkorff's induction coil. Here is a *monochord*, and a thin bar of iron stretches from one of its bridges to the other. This bar is placed in a glass tube, which is surrounded by copper wire. I place the contact breaker in a distant room, so that you cannot hear its noise. The current is now active, and every individual in this large assembly hears something between a dry crackle and a musical sound issuing from the bar in consequence of its successive magnetization and demagnetization.

Hitherto we have occupied ourselves with the iron which has been acted upon by the current. Let us now devote a moment's time to the examination of the current itself. Here is a naked copper wire which is quite inert, possessing no power to attract these iron filings. I send a voltaic current through it; it immediately grapples with the filings, and holds them round it in a thick envelope. I interrupt the current, and the filings fall. Here is a compact coil of copper wire which is overspun with cotton to prevent contact between the convolutions. At present the coil is inert; but now I send a current through it: a power of attraction is instantly developed, and you see that it is competent to empty this plate of iron nails.

* The sound, I find, was first noticed by Mr. Page.—J. T., 16th June.

Thus we have magnetic action exhibited by a body which does not contain a particle of the so-called magnetic metals. The copper wire is made magnetic by the electric current. Indeed, by means of a copper wire through which a current flows we may obtain all the effects of magnetism. I have here a long coil, so suspended as to be capable of free motion in a horizontal direction: it can move all round in a circle like an ordinary magnetic needle. At its ends I have placed two spirals of platinum wire which the current will raise to brilliant incandescence. They are glowing now, and the suspended coil behaves in all respects like a magnetic needle. Its two ends show opposite polarities: it can be attracted and repelled by a magnet, or by a current flowing through another coil; and it is so sensitive that the action of the earth itself is capable of setting it north and south.

There is an irresistible tendency to unify in the human mind; and, in accordance with our mental constitution, we desire to reduce phenomena which are so much alike to a common cause. Hence the conception of the celebrated Ampère that a magnet is simply an assemblage of electric currents. Round the atoms of a magnet Ampère supposed minute currents to circulate incessantly in parallel planes; round the atoms of common iron he also supposed them to circulate, but in all directions—thus neutralizing each other. The act of magnetization he supposed to consist in the rendering of the molecular currents parallel to a common plane, as they are supposed to be in a permanent magnet.

This is the celebrated theory of molecular currents propounded by Ampère. You observe it consists in the application of conceptions obtained from sensible masses of matter to insensible or atomic masses. Let us follow out this conception to what would appear its legitimate consequences. I have said that we obtain both attractions and repulsions from electric currents: all these effects are deduced from one law, which is, *that electric currents flowing in the same direction attract each other, while, when they flow in opposite directions, they repel each other*. Let me illustrate this law rapidly. Here are two flat coils suspended facing each other, and about eight inches apart. I send a current through both, causing it to flow through them in the same direction; the coils instantly clash and cling together in virtue of their mutual attraction. I now reverse the current through one of them, and they fly a yard asunder, in virtue of their mutual repulsion. And now one of them twists its suspending wire so as to turn its opposite face to the other coil; the currents are now again in the same direction, and the coils clash and cling as in the first instance. Imagine, then, our molecular currents flowing round the atoms of this iron bar in planes perpendicular to the length of the bar. From the law just enunciated we should infer the mutual attraction of those currents; and from this attraction we should be disposed to infer the *shortening* of the bar at the moment of magnetization. Here, for example, is a coil of copper wire suspended vertically; the end of the coil dips into this little basin of mercury. From a small voltaic battery behind I send a current through the coil; and, because it passes in the same

direction through all its convolutions, they attract each other. The coil is thereby shortened; its end quits the mercury with a spark; the current ceases; the wire falls by its own gravity; the current again passes, and the wire shortens as before. Thus you have this quick succession of brilliant* sparks produced by the shortening of the wire and the interruption of the current as it quits the mercury.

Is it a fact, then, that an iron bar is *shortened* by the act of magnetization? It is not. And here, as before, we enter into the labours of other men.

Mr. Joule was the first to prove that the bar is *lengthened*. Mr. Joule rendered this lengthening visible by means of a system of levers and a microscope, through which a single observer saw the action. The experiment has never, I believe, been made before a public audience; but the instrument referred to† at the commencement of this lecture will, I think, enable me to render this effect of magnetization visible to everybody present.

Before you is an upright iron bar, two feet long, firmly screwed into a solid block of wood. Sliding on two upright brass pillars is a portion of the instrument which you see above the iron bar. The essential parts of this section of the apparatus are, first, a vertical rod of brass, which moves freely and accurately in a long brass collar. The lower end of the brass rod rests upon the upper flat surface of the iron bar. To the top of the brass rod is attached a point of steel; and this point now presses against a plate of agate, near a pivot which forms the fulcrum of a lever. The distant end of the lever is connected, by a very fine wire, with an axis on which is fixed a small circular mirror. If the steel point be pushed up against the agate plate, the end of the lever is raised; the axis is thereby caused to turn, and the mirror rotates. I now cast a beam from an electric lamp upon this mirror; it is reflected in a luminous sheaf, fifteen or sixteen feet long, and it strikes our screen, there forming a circular patch of brilliant light. This beam is to be our index; it will move as the mirror moves, only with twice its angular velocity; and the motion of the patch of light will inform us of the lengthening and shortening of the iron bar.

I employ one battery simply to ignite the lamp. I have here a second battery to magnetize the iron bar. At present no current is passing. I make the circuit, and the bright image on the screen is suddenly displaced. It sinks a foot. I break the circuit: the bar instantly shrinks to its normal length, and the image returns to its first position. I make the experiment several times in succession: the result is always the same. Always when I magnetize, the image instantly descends, which declares the lengthening of the bar; always when I interrupt the current, the image immediately rises. A little warm water projected against the bar causes the image to descend

* Rendered brilliant by the introduction of a coil of wire and a core of soft iron into the circuit.

† Very skilfully constructed by Mr. Becker.

gradually. This, I believe, is the first time that this action of magnetism has been seen by a public audience.

I have employed the same apparatus in the examination of bismuth bars; and, though considerable power has been applied, I have hitherto failed to produce any sensible effect. It was at least conceivable that complementary effects might be here exhibited, and a new antithesis thus established between magnetism and diamagnetism.

No explanation of this action has, to my knowledge, been offered; and I would now beg to propose one which seems to be sufficient. I place this large flat magnet upon the table; over it I put a paper screen; and on the screen I shake iron filings. You know the beautiful lines in which those filings arrange themselves—lines which have become classical from the use made of them in this Institution; for they have been guiding-threads for Faraday's intelligence while exploring the most profound and intricate phenomena of magnetism. These lines indicate the direction in which a small magnetic needle sets itself when placed on any of them. The needle will always be a tangent to the magnetic curve. A little rod of iron, freely suspended, behaves exactly like the needle, and sets its longest dimension in the direction of the magnetic curve. In fact, the particles of iron filings themselves are virtually so many little rods of iron, which, when they are released from the friction of the screen by tapping, set their longest dimensions along the lines of force. Now, in this bar magnet the lines of force run *along the magnet itself*, and, were its particles capable of free motion, they also would set their longest dimensions parallel to the lines of force—that is to say, parallel to the length of the magnet. This, then, is the explanation which I would offer of the lengthening of the bar. The bar is composed of irregular crystalline granules; and, when magnetized, these granules tend to set their longest dimensions parallel to the axis of the bar. They succeed, partially, and produce a microscopic lengthening of the bar, which, suitably magnified, has been rendered visible to you.*

Perhaps you do not see the magnetic curves from your present position, but I will enable you to see them. I have here an electric lamp turned on its back, and from it a vertical cylinder of light now issues. Over the aperture of the lamp I place two small bar magnets, enclosed between two plates of glass. The vertical beam is received upon a looking-glass which reflects it on to the screen. In the path of this reflected beam I place a lens, and thus obtain upon the screen a magnified image of the two small bar magnets. And now I sprinkle this fine iron sand on the plate of glass, and you see how it arranges itself under the operation of the magnets. A most beautiful display

* My assistant, Mr. Barrett, has just drawn my attention to a paper by M. De la Rive in which this explanation is given. To him, therefore, belongs the entire credit of it.—J. T., June 16.

of the magnetic curves is now before you. And you observe, when I tap the glass, how the particles attach themselves by their ends, and how the curves close in upon each other. They try to attach themselves thus and close thus up in the solid iron bar: the consequence is that the longitudinal expansion is exactly counterbalanced by the transverse contraction, so that the *volume* of the bar remains unchanged.

But can we not bring a body with movable particles within an electro-magnetic coil? We can; and I will now, in conclusion, show you an experiment devised by Mr. Grove, which bears directly upon this question, but the sight of which, I believe, has hitherto been confined to Mr. Grove himself. At all events, I am not aware of its ever having been made before a large audience. I have here a cylinder with glass ends, and it contains a muddy liquid. This muddiness is produced by the magnetic oxide of iron which is suspended mechanically in water. Round the glass cylinder I have coiled five or six layers of covered copper wire; and here is a battery from which a current can be sent through the coil. First of all, I place the glass cylinder in the path of the beam from our electric lamp, and, by means of a lens, cast a magnified image of the end of the cylinder on the screen. That image at present possesses but feeble illumination. The light is almost extinguished by the suspended particles of magnetic oxide. But, if what I have stated regarding the lines of force through the bar of magnetized iron be correct, the particles of the oxide will suddenly set their longest dimensions parallel to the axis of the cylinder, and also in part set themselves end to end when the current is sent round them. More light will be thus enabled to pass; and now you observe the effect. The moment I establish the circuit the disc upon the screen becomes luminous: I interrupt the current, and gloom supervenes; I re-establish it, and we have a luminous disc once more.

The apparatus, as I have stated, was really invented to examine whether any mechanical effect of this kind could be detected in diamagnetic bodies; but hitherto without result. And this leads me to remark on the large ratio which the failures of an original inquirer bear to his successes. The public see the success—the failure is known to the inquirer alone. The encouragement of his fellow-men, it is true, often cheers the investigator and strengthens his heart; but his main trials occur when there is no one near to cheer him, and when, if he works aright, he must work for duty and not for reputation. And this is the spirit in which work has been executed in this Institution, by a man who has, throughout his life, turned a deaf ear to such allurements as this age places within the reach of scientific renown; and it behoves every friend of this Institution to join in the wish that *that man's* spirit may continue to live within its walls, and that those who come after him may not shrink from his self-denial while endeavouring to merit a portion of his fame.

[J. T.]

Friday, January 20, 1865.

H.R.H. THE COUNT OF PARIS, in the Chair.

PROFESSOR TYNDAL, F.R.S. M.R.I.

On Combustion by Invisible Rays.

WE are so accustomed to associate the word *ray* with the idea of light, that the term dark, or invisible, or obscure rays, stimulates the imagination by its strangeness; and such is more particularly the case when we are told that the major portion of the radiation of the sun itself is of this invisible character. This great discovery was announced sixty-five years ago by Sir William Herschel. Permitting a sun-beam to pass through a glass prism, he formed the coloured spectrum of the solar light; and, carrying a small thermometer through its various colours, he determined their heating power. He found this power to augment gradually from the violet to the red; but he also found that the calorific action did not terminate where the visible spectrum ended. Placing his thermometer in the dark space beyond the red, he proved the heating power there to be greater than in any part of the visible spectrum.

Sir William Herschel concluded from his experiments, that besides those rays which, acting separately upon the retina, produce the sensation of colour, and the sum of which constitutes our ordinary sunshine, a vast outflow of perfectly invisible rays proceeds from the sun, and that, measured by their heating power, the strength or energy of these invisible rays is greater than that of all the visible rays taken together.

This result was questioned by some and confirmed by others; but, like every natural truth that can be brought to the test of experiment, the verity of Sir William Herschel's announcement was soon completely established. Forty years after the discovery of those invisible rays by his father, Sir John Herschel made them the subject of experiment. He made an arrangement which enabled him to estimate the heating power of the spectrum by its *drying* power. Wetting by a wash of alcohol, paper blackened on one side, he cast his spectrum on this paper, and observed the chasing away of the moisture by the heat of the rays. His drying paper presented to him a *thermograph* of the spectrum and showed the heating power to extend far beyond the red.

By the introduction of the thermo-electric pile, Melloni created a new epoch in researches on radiant heat. This instrument enables us to examine, with a precision unattainable with ordinary thermometers, the distribution of heat in the solar spectrum. Melloni himself de-

voted some time to this subject. He had made the discovery that various substances, in the highest degree transparent to light, were eminently opaque to those invisible heat-rays. Pure water, for example, is a body of this kind. Only one substance did Melloni find to be equally pervious to the visible and the invisible rays—namely, transparent rock-salt. And though the researches of MM. De la Provostaye and Desains, together with some extremely suggestive experiments executed by Mr. Balfour Stewart, show conclusively that Melloni erred in supposing rock-salt to be *perfectly* transparent, it must be admitted that, in this respect, the substance approaches very near perfection.

Abandoning prisms of glass, which had been always employed previously, Melloni made use of a prism of rock-salt in his experiments on the solar spectrum. He was thus enabled to prove that the ultra-red rays discovered by Sir William Herschel formed an invisible spectrum, at least as long as the visible one. He also found the position of maximum radiant power to lie as far on one side the red as the green light of the spectrum on the other.

Dr. Franz, of Berlin, subsequently examined the distribution of heat in the solar spectrum, employing for this purpose a flint-glass prism. He showed that the inaction of the ultra-red rays upon the retina did not altogether arise from the absorption of those rays in the humours of the eye; at all events, he proved that a sensible portion of the invisible rays was transmitted across the eye-ball of an ox,—and reached the back of the eye. Professor Muller, of Freiberg, afterwards examined very fully the heat of the solar spectrum; and representing, as Sir William Herschel also had approximately done, by lines of various lengths the thermal intensity at various points, he drew a curve which expressed the calorific action of the entire spectrum.

At various intervals during the last ten years the speaker had occupied himself with the invisible radiation of the electric light; and to the distribution of heat in its spectrum he now directed attention. The instruments made use of were the electric lamp of Duboscq and the linear thermo-electric pile of Melloni. The spectrum was formed by means of lenses and prisms of pure rock-salt. It was equal in width to the length of the row of elements forming the pile, and the latter being caused to pass through its various colours in succession, and also to search the space right and left of the visible spectrum, the heat falling upon it, at every point of its march, was determined by the deflection of an extremely sensitive galvanometer.

As in the case of the solar spectrum, the heat was found to augment from the violet to the red, while in the dark space beyond the red it rose to a maximum. The position of the maximum was about as distant from the extreme red in the one direction, as the green of the spectrum in the opposite one.

The augmentation of temperature beyond the red in the spectrum of the electric light is sudden and enormous. Representing the thermal intensities by lines of proportional lengths, and erecting these lines as

perpendiculars at the places to which they correspond, when we pass beyond the red these perpendiculars suddenly and greatly increase in length, reach a maximum, and then fall somewhat more suddenly on the opposite side of the maximum. When the ends of the perpendiculars are united, the curve beyond the red, representing the obscure radiation, rises in a steep and massive peak, which quite dwarfs by its magnitude the radiation of the luminous portion of the spectrum.

Interposing suitable substances in the path of the beam, this peak may be in part cut away. Water, in certain thicknesses, does this very effectually. The vapour of water would do the same; and this fact enables us to account for the difference between the distribution of heat in the solar and in the electric spectrum. The comparative height and steepness of the ultra-red peak, in the case of the electric light, are much greater than in the case of the sun, as shown by the diagram of Professor Müller. No doubt the reason is, that the eminence corresponding to the position of maximum heat in the solar spectrum has been cut down by the aqueous vapour of our atmosphere. Could a solar spectrum be produced beyond the limits of the atmosphere, it would probably show as steep a mountain of invisible rays as that exhibited by the electric light, which is practically uninfluenced by atmospheric absorption.

Having thus demonstrated that a powerful flux of dark rays accompanies the bright ones of the electric light, the question arises, "Can we not detach the former and experiment on them alone?"

One way of doing this would be to cut off the luminous portion of the decomposed beam by an opaque screen, allowing the non-luminous portion to pass by its edge. We might then operate at pleasure upon the latter:—reflect it, refract it, concentrate it. This, in fact, was done by Sir William Herschel, but a quantity of heat could not thus be obtained sufficient to produce the results intended to be exhibited before the conclusion of the discourse. Another plan consists in permitting the total radiation to pass through some substance transparent to the heat rays, but opaque to the light rays. Melloni discovered that lampblack, and also a kind of black glass, while perfectly opaque to light, transmitted a considerable quantity of radiant heat. In the "Lectures on Heat," given at the Royal Institution in 1862, and since made public, experiments with these bodies are described. It was while conversing with his friend Mr. Warren De la Rue, in the autumn of 1861, on the possibility of sifting, by absorbents, the light of a beam from its heat, that the speaker first learned that carbon was the substance which rendered Melloni's glass opaque. This fact was of peculiar interest to him, for it and others seemed to extend to solid bodies a law which he had detected two years previously in his experiments on gases and vapours, and which showed that *elementary* gases were highly transparent, while *compound* gases were all more or less opaque—many of them, indeed, almost perfectly opaque—to invisible radiant heat.

In the speaker's first experiments on the invisible radiation of

the electric light, black glass was the substance made use of. The specimens, however, which he was able to obtain destroyed, along with the visible, a considerable portion of the invisible radiation.* But the discovery of the deportment of elementary gases directed his attention to other simple substances. He examined sulphur dissolved in bisulphide of carbon, and found it almost perfectly transparent to the invisible rays. He also examined the element bromine, and found that notwithstanding its dark colour, it was eminently transparent to the ultra-red rays. Layers of this substance, for example, which entirely cut off all the light of a brilliant gas flame, transmitted its invisible radiant heat with freedom. Finally, he tried a solution of iodine in bisulphide of carbon, and arrived at the extraordinary result, that a quantity of dissolved iodine sufficiently opaque to cut off the light of the mid-day sun was, within the limits of experiment, absolutely transparent to invisible radiant heat.

This then is the substance by which the invisible rays of the electric light may be almost perfectly detached from the visible ones. Concentrating by a small glass mirror, silvered in front, the rays emitted by the carbon points of the electric lamp, we obtain a convergent cone of light. Interposing in the path of this concentrated beam a cell containing the opaque solution of iodine, the light of the cone is utterly destroyed, while its invisible rays are scarcely, if at all, meddled with. These converge to a focus, at which, though nothing can be seen even in the darkest room, the following series of effects may be produced:—

When a piece of black paper is placed in the focus, it is pierced by the invisible rays, as if a white-hot spear had been suddenly driven through it. The paper instantly blazes, without apparent contact with anything hot.

A piece of brown paper placed at the focus soon shows a red-hot, burning surface, extending over a considerable space of the paper, which finally bursts into flame.

The wood of a hat-box similarly placed, is rapidly burnt through. A pile of wood and shavings, on which the focus falls, is quickly ignited, and thus a fire may be set burning by the invisible rays.

A cigar or a pipe is immediately lighted when placed at the focus of invisible rays.

Disks of charred paper placed at the focus are raised to brilliant incandescence; charcoal is also ignited there.

A piece of charcoal, suspended in a glass receiver full of oxygen, is set on fire at the focus, burning with the splendour exhibited by this substance in an atmosphere of oxygen. The invisible rays, though they have passed through the receiver, still retain sufficient power to render the charcoal within it red-hot.

* The glass in thin layers had a greenish hue: I have since found black glass far more diathermic.—J. T.

A mixture of oxygen and hydrogen is exploded in the dark focus, through the ignition of its envelope.

A strip of blackened zinc-foil placed at the focus is pierced and inflamed by the invisible rays. By gradually drawing the strip through the focus, it may be kept blazing with its characteristic purple light for a considerable time. This experiment is particularly beautiful.

Magnesium wire, presented suitably to the focus, burns with almost intolerable brilliancy.

The effects thus far described are, in part, due to chemical action. The substances placed at the dark focus are oxidizable ones, which, when heated sufficiently, are attacked by the atmospheric oxygen, ordinary combustion being the result. But the experiments may be freed from this impurity. A thin plate of charcoal, placed *in vacuo*, is raised to incandescence at the focus of invisible rays. Chemical action is here entirely excluded. A thin plate of silver or copper, with its surface slightly tarnished by the sulphide of the metal, so as to diminish its reflective power, is raised to incandescence either *in vacuo* or in air. With sufficient battery-power and proper concentration, a plate of platinized platinum is rendered white-hot at the focus of invisible rays; and when the incandescent platinum is looked at through a prism, its light yields a complete and brilliant spectrum. In all these cases we have, in the first place, a perfectly invisible image of the coal points formed by the mirror; and no experiment hitherto made illustrates the identity of light and heat more forcibly than this one. When the plate of metal or of charcoal is placed at the focus, the invisible image raises it to incandescence, and thus prints itself visibly upon the plate. On drawing the coal points apart, or on causing them to approach each other, the thermograph of the points follows their motion. By cutting the plate of carbon along the boundary of the thermograph, we might obtain a second pair of coal points, of the same shape as the original ones, but turned upside down; and thus by the rays of the one pair of coal points, which are incompetent to excite vision, we may cause a second pair to emit all the rays of the spectrum.

The ultra-red radiation of the electric light is known to consist of ethereal undulations of greater length, and slower periods of recurrence, than those which excite vision. When, therefore, those long waves impinge upon a plate of platinum, and raise it to incandescence, their period of vibration is changed. The waves emitted by the platinum are shorter and of more rapid recurrence, than those falling upon it; the refrangibility being thereby raised, and the invisible rays rendered visible. Thirteen years ago, Professor Stokes proved that by the agency of sulphate of quinine, and various other substances, the ultra-violet rays of the spectrum could be rendered visible. These invisible rays of high refrangibility, impinging upon a proper medium, cause the molecules of that medium to oscillate in slower periods than those of the incident waves. In this case, therefore, the invisible rays are rendered visible by the *lowering* of their refrangibility; while in the experiments of the speaker, the ultra-red rays are rendered visible

by the *raising* of their refrangibility. To the phenomena brought to light by Professor Stokes, the term *fluorescence* has been applied by their discoverer, and to the phenomena brought forward this evening at the Royal Institution, it was proposed to apply the term *calorescence*.

It was the discovery, more than three years ago, of a substance opaque to light, and almost perfectly transparent to radiant heat—a substance which cut the visible spectrum of the electric light sharply off at the extremity of the red, and left the ultra-red radiation almost untouched, that led the speaker to the foregoing result. They lay directly in the path of his investigation; and it was only the diversion of his attention to subjects of more immediate interest that prevented him from reaching, much earlier, the point which he has now attained. On this, however, he can found no claim; and the *idea* of rendering ultra-red rays visible, though arrived at independently, does not by right belong to him. The right to a scientific idea or discovery is secured by the act of publication; and, in virtue of such an act, priority of conception as regards the conversion of heat-rays into light-rays belongs indisputably to Dr. Akin. At the meeting of the British Association, assembled at Newcastle in 1863, he proposed three experiments by which he intended to solve this question. He afterwards became associated with an accomplished man of science, Mr. Griffith of Oxford, and jointly with him pursued the inquiry. Two out of the three experiments proposed at Newcastle by Dr. Akin are impracticable. In the third, Dr. Akin proposed to converge the rays of the sun by a concave mirror, to cut off the light by ‘proper absorbents,’ and to bring platinum foil into the focus of invisible rays. It is quite possible, that, had he possessed the instrumental means at the speaker’s disposal, or had he been sustained as the speaker had been both by the Royal Society and the Royal Institution, Dr. Akin might have been the first to effect the conversion of the dark heat-rays into luminous ones. For many years the idea of forming an intense focus of invisible rays had been perfectly clear before the speaker’s mind; and in 1862 he published experiments upon the subject. The effects observed by him in 1862 at the focus of invisible rays were such as no previous experimenter had witnessed, and no experimenter could have observed them without being driven to the results which formed the subject of the evening’s discourse. Still publication is the sole test of scientific priority; and it cannot be denied that Dr. Akin was the first to propose definitely to change the refrangibility of the ultra-red rays of the spectrum, by causing them to raise platinum foil to incandescence.

[J. T.]

Friday, February 3, 1865.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. Vice-President,
in the Chair.

WILLIAM ODLING, M.B. F.R.S.

On Aluminium Ethide and Methide.

THE symbols by which the atomic proportions of a few of the principal metallic elements are usually represented, together with the relative weights of these several proportions, are shown in the following table :—

Lithium	Li	7
Magnesium	Mg	24
Zinc	Zn	65
Arsenic	As	75
Silver	Ag	108
Tin	Sn	118
Mercury	Hg	200
Lead	Pb	207
Bismuth	Bi	210

It is observable that the atomic proportions of the metals range from 7 parts of lithium, through 108 parts of silver up to 210 parts of bismuth. Now it is found that all these different proportions have substantially the same specific heat, so that 7 parts of lithium, 108 parts of silver, and 210 parts of bismuth, for instance, absorb or evolve the same amount of heat in undergoing equal increments or decrements of temperature. Hence, taking silver as a convenient standard of comparison, the atomic proportion of any other metal may be defined to be, that quantity of the metal which has the same specific heat as 108 parts of silver.

Many of the metals unite with the halogen radicles chlorine and bromine, as also with the organic radicles ethyl and methyl, to form volatile compounds, which may be conveniently compared with the chloride and ethide of hydrogen. Now, it is found that the several proportions of metal or hydrogen contained in equal volumes of these gaseous chlorides or ethides, are their respective atomic proportions; so that equal volumes of chloride or ethide of hydrogen, zinc, arsenic, tin, mercury, lead, and bismuth, for instance, contain 1, 65, 75, 118, 200, 207, and 210 parts of hydrogen or metal respectively. Hence, the molecule of chloride of hydrogen, HCl, being conventionally regarded as constituting two volumes, the atomic proportion of a metal may be defined to be that quantity of the metal

which is contained in two volumes of its gaseous chloride, or bromide, or ethide, or methide, &c.

These two definitions having reference respectively to the specific heats of the metals, and the molecular volumes of their gaseous compounds, lead in all cases to the same conclusion. Thus, 200 parts of mercury is the quantity of mercury which has the same specific heat as 108 parts of silver, and is also the quantity of mercury contained in two volumes of mercuric chloride, mercuric ethide, &c.

The atomic proportions of the different metals unite with 1, 2, 3, 4, &c. atoms of chlorine and ethyl, to form the two-volume molecules of their respective chlorides and ethides, as shown below :—

2 Vols	2 Vols	2 Vols.
H Cl	H Et	"
HgCl ₂	HgEt ₂	ZnEt ₂
Bi Cl ₃	Bi Et ₃	AsEt ₃
Sn Cl ₄	Sn Et ₄	PbEt ₄

Or, two volumes of the gaseous chlorides of hydrogen, mercury, bismuth, and tin, for instance, are found to contain respectively 35.5 parts, twice 35.5 parts, three times 35.5 parts, and four times 35.5 parts of chlorine.

Aluminium, which is one of the three most abundant constituents of the earth's crust, and the most abundant of all its metallic constituents, enters into the composition of a large number of native minerals of great value in the fine and useful arts, and also forms extremely well-defined artificial compounds, possessing a high degree of chemical interest. Nevertheless chemists are not at all agreed as to the atomic weight which should be accorded to the metal, or as to the molecular formulae of its principal compounds.

The quantity of aluminium which has the same specific heat as 108 parts of silver, is found to be 27.5 parts; and analysis shows that this quantity of aluminium combines with three times 35.5 parts of chlorine to form chloride of aluminium. Accordingly the atomic proportion of aluminium should be fixed at 27.5 parts; its chloride be formulated as a trichloride thus, AlCl₃; and its other compounds be represented by corresponding expressions, as shown in the left-hand column of the following table, instead of by the heretofore used more complex expressions shown in the right-hand column :—

Al 27.5		Al 13.75
AlCl ₃	Chloride	Al ₂ Cl ₃
Na AlCl ₄	Sodio-Chloride	Na Al ₂ Cl ₅
Na ₃ AlF ₆	Cryolite	Na ₃ Al ₂ F ₆
Na ₃ AlO ₃	Aluminate	Na ₃ Al ₂ O ₃
H AlO ₂	Diaspore	H Al ₂ O ₃
K AlS ₂ O ₆	Alum	K Al ₂ S ₂ O ₆
K AlSi ₃ O ₈	Feldspar	K Al ₂ Si ₃ O ₈
P AlO ₄	Phosphate	P Al ₂ O ₄

But the quantity of aluminium contained in two volumes of its gaseous chloride was found by Deville to be 55 parts, instead of 27.5 parts, while the quantity of chlorine was found to be *six* times 35.5 parts, instead of *three* times 35.5 parts. Hence, relying exclusively upon molecular volume, the atomic weight of aluminium would be 55, and the formula of chloride of aluminium $AlCl_3$. This conclusion, however, is inadmissible for several reasons, and chiefly because it would make the atomic proportion of aluminium possess a specific heat twice as great as that belonging to the atomic proportion of any other metal.

To evade this difficulty some chemists have proposed to accord to the molecule of aluminic chloride the formula Al_2Cl_6 , whereby an indivisible proportion of metal would be habitually represented by a divisible symbol; for it is agreed on all hands that the proportion of aluminium contained in the molecule of aluminic chloride is the smallest proportion of aluminium found in any aluminic compound whatsoever; that it is incapable of experimental division by any process whatsoever; and consequently that, so far as our present knowledge goes, it is an indivisible or atomic proportion.

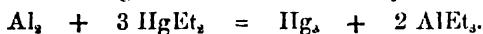
Now there are undoubtedly certain bodies, elementary and compound, of which the ascertained vapour densities, and consequent volumes, no matter how accounted for, are, as a mere matter of experiment, discordant with the chemical analogies of the respective bodies; but in most instances these anomalous results are rendered unimportant by other determinations of vapour density, either of the same bodies raised to higher temperatures, or of associated bodies having a more decided volatility. Hence arises the question whether the ascertained volume of aluminic chloride, which is discordant with the specific heat of aluminium, may not be anomalous in a similar manner, and whether the anomaly may not be corrected by an examination of other more volatile aluminic compounds.

The methide and ethide of aluminium recently obtained by Mr. Buckton and the speaker are, so to speak, varieties of aluminic chloride in which the chlorine has been replaced by methyl and ethyl, and are at the same time far more volatile and manageable than the typical chloride. Now, it has been found that two gaseous volumes of the methide and ethide of aluminium contain only 27.5 parts of aluminium, united with three atomic proportions of methyl and ethyl; and accordingly their molecules have to be expressed by the formulæ $AlMe_3$ and $AlEt_3$, respectively. In other words, the normal results obtained with the methide and ethide correct the anomalous result obtained with the chloride, and confirm the atomic weight and molecular formulæ deducible from the specific heat of aluminium.

That the ascertained vapour density of aluminic chloride is really anomalous receives a further corroboration in the behaviour of aluminic methide itself. At 220° , and all superior temperatures, the vapour density of this compound shows that two volumes of its vapour contain 27.5 parts of aluminium and *three* times 15 parts of

methyl; but at 130° , its vapour density, corrected for alteration of temperature, becomes very nearly doubled, or, in other words, two volumes of its vapour contain very nearly 55 parts of aluminium and *six* times 15 parts of methyl. According, however, to the well-known rule, based on the separate researches of Cahours and Deville, the molecular formula of a body must be calculated from its permanent or ultimate, and not from its variable or initial vapour density, whence the high vapour density of aluminium methide at 130° does not at all interfere with our attributing to its molecule the formula AlMe_2 , deducible from its vapour density at 220° and upwards, and harmonizing with the specific heat of metallic aluminium.

Aluminium ethide and methide occur as colourless liquids. The ethide boils at 194° , and does not freeze at -18° . The methide boils at 130° , and solidifies at a little above 0° into a beautiful crystalline mass. Both liquids take fire on exposure to air, and explode violently by contact with water. They are produced from mercuric ethide and methide respectively, by heating these compounds for some hours in a water-bath, with excess of aluminium clippings. This process was obviously suggested by Frankland and Duppa's new reaction for making zinc ethide, methide, amylide, &c.



[W.O.]

Friday, April 7, 1865.

H.R.H. THE PRINCE OF WALES, Vice-Patron, in the Chair.

DR. A. W. HOFMANN, F.R.S.

On the Combining Power of Atoms.

YOUR ROYAL HIGHNESS, LADIES AND GENTLEMEN,

You observe on the lecture-table a great number of bottles, containing an almost perplexing variety of chemical substances; the walls of the theatre are covered with diagrams exhibiting an endless number of formulæ, which some of my audience, I have no doubt, are contemplating with mixed feelings of uneasiness and resignation. Nor will it diminish your discomfort if I tell you that each of the substances on the table represents at least a thousand bodies actually known, and again that each of the known bodies represents a million or more bodies which are not known, but which exist well defined in the mind of the chemist who calls them into life whenever he requires them for purposes theoretical or practical. At the first glance, I admit this is rather a formidable array; but our anxiety is materially lessened, if we learn that this host of substances is formed according to simple laws which it is in our power to discover, and which by the united efforts of chemists are gradually unfolding themselves.

May I be permitted this evening to call your attention to some of the laws, or at all events law-like regularities, recently observed, and which are at present engrossing the interest of the leading chemists of all countries.

I would commence with one of the simplest of experiments:—Here are two gases, hydrogen and chlorine, the one a colourless inodorous gas, which burns quietly with a pale flame, the other a greenish yellow gas, possessing a suffocating odour, and not inflammable. When equal volumes of these two gases are mixed together, and a light applied to

the mixture, an explosion takes place, a compound gas being produced, which fumes in the air, and which, when dissolved in water, constitutes the spirit of salt, or muriatic acid of the earlier chemists, now called *hydrochloric acid*.

By certain processes, which we must not inquire into at the present time, the compound of hydrogen and chlorine, called hydrochloric acid, may be made to unite with another gas, oxygen; the combination giving rise to the formation of an acid called *hypochlorous acid*, and by which most of the bleaching operations carried out in commerce are effected. On plunging an appropriately-prepared piece of Turkey-red calico into a warm solution of chloride of lime, which is closely related to hypochlorous acid, you observe that a white pattern is produced on the cloth.

Again, an additional quantity of oxygen may be united to hypochlorous acid, when the compound called *chlorous acid* is formed. A well-known salt of commerce, chlorate of potassium, is generally used for its preparation. This salt is readily decomposed by acids. The explosive violence with which sulphuric acid acts upon it, the detonation attended by flashes of light, are phenomena early and frequently observed by the laboratory student. By substituting nitric for sulphuric acid, an explosive gas is formed, which, when dissolved in water, constitutes the chlorous acid in question.

A third compound, *chloric acid*, may be formed by the addition of a further dose of oxygen to chlorous acid. The most familiar salt of this acid is the chlorate of potassium just mentioned. This, as well as other derivatives of chloric acid, is largely used for pyrotechnic purposes. Here we must be satisfied with calling attention to a species of white gunpowder which has been lately devised, and which consists of a mixture of the potassium salt with tannic or gallic acid.

Lastly, by still further increasing the supply of oxygen, a fourth compound, *perchloric acid*, is produced. This substance again exhibits the explosive habitudes of the group of bodies in question. When made to combine with aniline, for instance, perchloric acid gives rise to a compound which goes off when heated in a test-tube, burning, as you observe, with an intense white light.

A glance at the diagram shows us the regularity with which the amount of oxygen increases in this series of oxygenetted derivatives of hydrochloric acid. There is nothing arbitrary in this accession; each increment ensues step by step.

Increment of Oxygen.

Hydrochloric acid	. .	HCl
Hypochlorous acid	. .	HCl + O = HClO
Chlorous Acid	. . .	HCl + 2 O = HClO ₂
Chloric Acid	HCl + 3 O = HClO ₃
Perchloric Acid	HCl + 4 O = HClO ₄

Looking at the simplest body at the head of the list, we find it to be a compound of one atom of hydrogen with one atom of chlorine. By uniting this compound with one atom of oxygen, we obtain hypochlorous acid; by adding another, we produce chlorous acid; the accession of a third atom gives rise to chloric acid; and a fourth atom of oxygen, lastly, produces perchloric acid.

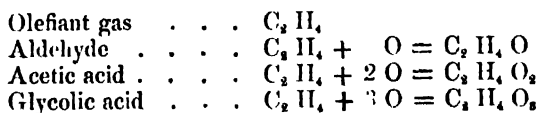
The same gradual rise in the successive additions of oxygen may be traced in numerous other groups of bodies. For the present we will confine our attention to two more series.

Under the name of *olefiant gas*, a transparent colourless gas, burning, as you observe, with a brilliantly luminous flame, is known, which consists of carbon and hydrogen. By a circuitous process, which must not detain us, this gas may be converted into a peculiar, very volatile liquid of a pungent odour, which chemists have called *aldehyde*. This substance, which is more readily procured by submitting alcohol to the action of oxidizing agents, may be looked upon as a compound of olefiant gas with oxygen. Aldehyde is marked by its powerful attraction for oxygen; so greedily, indeed, does it absorb this gas, that it is almost impossible to preserve aldehyde except in hermetically-closed vessels. Nor does aldehyde unite only with oxygen when it meets with it in the free state; combined oxygen is equally attracted by it. When gently heated with oxide of silver, dissolved in a suitable liquid, aldehyde very rapidly removes the oxygen from the oxide, metallic silver in a lustrous condition being deposited on the surface of the vessel in which the operation is performed. This reaction, first observed by Baron Liebig many years ago, has recently been modified so as to admit of industrial application on a large scale for the manufacture of looking-glasses and reflectors for astronomical instruments. When aldehyde is thus oxidized, it is converted into a compound called *acetic acid*, well known to everyone as the principal constituent of common vinegar.

Again, acetic acid may be made to unite with an additional quantity of oxygen to produce a compound called *glycolic acid*, an acid which has been discovered during the last few years, but which has not at present received any useful applications.

On referring to the following diagram we perceive the simple relation existing between these four bodies :—

Increment of Oxygen.



We observe here exactly as in the previous series the gradual

assimilation of oxygen. Olefiant gas by absorbing one atom of oxygen produces aldehyde; by absorbing a second atom it gives rise to the formation of acetic acid; by a third annexation of a similar quantity of oxygen, lastly, glycolic acid is formed.

May I be permitted to quote an additional instance. In the oil obtained from the seeds of the *croton tiglium* chemists have discovered a peculiar acid, *crotonic acid*, which has been lately observed also among the derivatives of mustard.

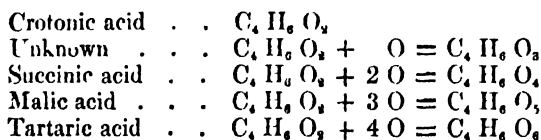
This substance, consisting of carbon, hydrogen, and oxygen, is convertible by oxidation into another acid, *succinic acid*, a beautiful crystalline body, which is more frequently obtained by submitting the familiar resin amber to the action of oxidizing agents.

Succinic acid, when combined with an additional atom of oxygen, gives rise to the formation of *malic acid*, a crystalline acid largely present in the juice of apples and rhubarb, the substance indeed to which the acid reaction of these juices is chiefly due. On cutting an apple or a piece of rhubarb, and pressing the cut surface against a piece of blue litmus paper, it is immediately reddened.

But malic acid also may be still further oxidized, the product being one of the most familiar vegetal acids, *tartaric acid*. This acid is one of the compounds existing largely in the juice of grapes. When a grape is squeezed on a piece of blue litmus paper, the latter is reddened wherever it comes in contact with the grape juice.

The molecules of all these acids contain the same number of carbon atoms and also the same number of hydrogen atoms, the difference in their composition consisting entirely in the number of oxygen atoms which are present, as obvious by reference to the diagram.

Increment of Oxygen.



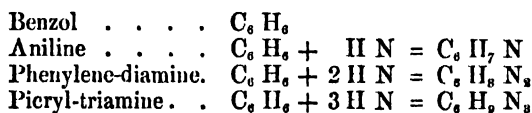
In this diagram an unknown acid $C_4 H_6 O_3$ figures between crotonic and succinic acids. This substance has not yet been obtained, but the experience of the gradual assimilation of oxygen in other series permits us to forecast the existence of this compound. Though not yet actually prepared, I have not hesitated to introduce it into the list of bodies derived from crotonic acid by simple oxidation.

The three examples which we have studied unmistakably show us that oxygen is capable of combining with other groups of elements

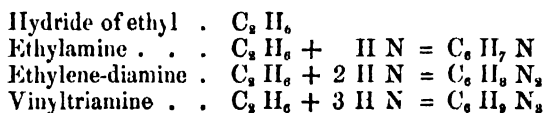
so as to give rise to new compounds: that this combination takes place stepwise, atom by atom: that the fundamental properties of the original compound remain more or less unaltered in the new compound of greater complexity, and that the amount of oxygen thus, so to say, assimilated, is in no way dependent on the greater or less complexity of composition of the original compound. In the first case we had the simplest of all possible compounds, hydrochloric acid consisting of one atom of hydrogen and one of chlorine; in the second case we started with a compound containing two atoms of carbon and four of hydrogen, altogether six atoms, while in the case of crotonic acid not less than four atoms of carbon, six of hydrogen, and two of oxygen, altogether twelve atoms, were involved.

An endless variety of similar examples might be quoted for the purpose of illustrating the generality of the inferences we have drawn, but I will, with your permission, assume that I have established my point.

We have in the next place to examine whether substances are capable of combining with nitrogen, exactly as we have just seen them unite with oxygen. Remembering as we do that nitrogen is rather marked by the absence of salient combining powers, we are not surprised to learn that all attempts hitherto made with the view of adding nitrogen directly to other bodies have entirely failed. But even indirectly by availing ourselves of roundabout ways, by calling to our aid the multitude of reactions which modern chemistry has brought to light, nitrogen cannot be *added* to other substances without introducing at the same time other elements into the composition of the compound. Let us endeavour to illustrate this point by examples: we could not perhaps quote a better case than that of *benzol*, the substance which is the starting point of the manufacture of aniline, the source of the beautiful colours so much in request at the present time. Benzol consists of carbon and hydrogen. No one has, as yet, succeeded in uniting this substance with nitrogen alone. Nothing on the other hand is easier than to combine benzol simultaneously with nitrogen and hydrogen. The very transition of benzol into *aniline* involves the assimilation by the benzol molecule of one atom of nitrogen and one atom of hydrogen. Again, aniline is capable of fixing a second atom of nitrogen, but not without assimilating also a second atom of hydrogen. The compound thus produced is a beautiful crystalline body called *phenylene-diamine*, which is likely to receive some interesting applications in the manufacture of brown dyes. To this compound again additional atoms of nitrogen and hydrogen may be joined, a fourth substance, as yet without application, *picryl-triamine*, being formed. The following diagram, in which again the simplest compound (benzol) is placed at the top of the list, shows how these several substances are related to each other.

Increment of Nitrogen.

Let me give you another and even simpler series in illustration of the same point. Hydride of ethyl, like benzol, refuses to combine with nitrogen, but it also receives into its molecule one atom of nitrogen and one atom of hydrogen, the well-known substance *ethylamine*, which has the greatest analogy with ammonia, being formed. This, by a repetition of the same transaction, is converted into *ethylene-diamine*, an oily base of great causticity; while a third repetition of the process produces a compound, *vinyl-triamine*, the existence of which is not yet fully established. The analogy between the first and the second series is obvious by a comparison of the formulæ.

Increment of Nitrogen.

We entirely refrain from examining into the particular processes, varying to a very considerable extent, by which these transformations are accomplished, the only point, which we have an interest in establishing here, being that nitrogen, when it joins a compound, joins not alone, but in company with hydrogen. In this respect then nitrogen essentially differs from oxygen, which we saw combining with bodies, atom by atom, without involving the introduction of other materials.

Can we explain this strange difference in the behaviour of oxygen and nitrogen? Before endeavouring to answer this question, let us examine in what manner carbon atoms are received into the molecules of bodies, whether like oxygen atoms they are capable of joining directly, or like nitrogen atoms they are accepted only when presenting themselves in company with other atoms. Investigation of a special case appears best fitted to supply the desired information.

Among the endless number of carbon compounds, we could not possibly select a simpler one than *marsh-gas*. This transparent colourless inflammable gas, as everyone knows, escapes from the fissures of the great coal measures and accumulates in the gal-

series of ill-ventilated coal mines, frequently giving rise to the explosions so much dreaded and deplored. It is also often developed from stagnant pools and swamps, in general from marshy lands, whence its name. Marsh-gas consists of carbon and hydrogen. Is this substance convertible into a compound which contains a larger amount of carbon? By a series of processes far too numerous and complicated to admit of being discussed this evening, marsh-gas may be converted into *hydride of ethyl*, a substance of very similar properties, and which the members of the Royal Institution have frequently seen prepared by a simpler method discovered by Dr. Frankland, *viz.*: the action of zinc-ethyl upon water. Hydride of ethyl contains one atom of carbon more than marsh-gas, but with this carbon atom, two hydrogen atoms have been simultaneously conferred upon the marsh-gas molecule. On submitting hydride of ethyl to a similar series of transformations we convert it by the addition of another carbon atom into *hydride of propyl*, but not without fixing again two atoms of hydrogen.

The same processes may be repeated again and again, hydride of propyl being converted in its turn into *hydride of butyl*, and hydride of butyl into *hydride of amyl*. We arrive in this manner at a series of bodies very similar in their properties, each of them differing from the previous one by the accession of one carbon atom invariably linked with two atoms of hydrogen. Many members of this series are found amongst the products of the distillation of coal; others, especially those richer in carbon, existing in the American oils, which are now being so much used for lighting and other purposes.

The composition of these several bodies may be exhibited in the following diagram:—

Increment of Carbon.

HYDRO-CARBONS.

Marsh-gas	CH_4
Hydride of ethyl	$\text{CH}_4 + \text{CH}_2 = \text{C}_2\text{H}_6$
Hydride of propyl . . .	$\text{CH}_4 + 2 \text{CH}_2 = \text{C}_3\text{H}_8$
Hydride of butyl	$\text{CH}_4 + 3 \text{CH}_2 = \text{C}_4\text{H}_{10}$
Hydride of amyl	$\text{CH}_4 + 4 \text{CH}_2 = \text{C}_5\text{H}_{12}$
Hydride of caproyl . . .	$\text{CH}_4 + 5 \text{CH}_2 = \text{C}_6\text{H}_{14}$
Hydride of cnanthyl . . .	$\text{CH}_4 + 6 \text{CH}_2 = \text{C}_7\text{H}_{16}$
Hydride of capryl	$\text{CH}_4 + 7 \text{CH}_2 = \text{C}_8\text{H}_{18}$

But we may illustrate the law which regulates the increment of carbon by starting from another foundation. Instead of building on marsh-gas, we make use of the oxide of marsh-gas, *methylic alcohol*. This compound by the successive addition of one atom of carbon and

two of hydrogen, produces a series of alcohols which may be regarded as the oxides of the corresponding marsh-gas-analogous. The first compound thus obtained is *ethylic alcohol*, ordinary spirits of wine; the second, *propylic alcohol*, generated in the fermentation of the grape-skin residue of the manufacture of wine; the third, *butylic alcohol*, formed by the fermentation of the molasses of beetroot sugar; the fourth, *amylic alcohol*, or potato oil, obtained as a residue in the manufacture of spirit from the starch of potatoes. *Caproic*, *ænanthylic*, and *caprylic*, *alcohols* are further terms of the series, which rises, not without considerable gaps, to terms containing as many as eighteen, twenty-seven, and even thirty atoms of carbon, which are found respectively in *palmitic*, *cerotic*, and *melissic alcohols*: the first, a product of the decomposition of spermaceti; the last two, derived from ordinary bee's wax and Chinese wax.

Increment of Carbon.

ALCOHOLS.

Methylic alcohol	.	CH_3O	
Ethylic	„	$\text{CH}_3\text{O} + \text{CH}_2 = \text{C}_2\text{H}_5\text{O}$	
Propylic	„	$\text{CH}_3\text{O} + 2 \text{CH}_2 = \text{C}_3\text{H}_7\text{O}$	
Butylic	„	$\text{CH}_3\text{O} + 3 \text{CH}_2 = \text{C}_4\text{H}_{10}\text{O}$	
Amylic	„	$\text{CH}_3\text{O} + 4 \text{CH}_2 = \text{C}_5\text{H}_{13}\text{O}$	
Caproylic	„	$\text{CH}_3\text{O} + 5 \text{CH}_2 = \text{C}_6\text{H}_{15}\text{O}$	
Ænanthylic	„	$\text{CH}_3\text{O} + 6 \text{CH}_2 = \text{C}_7\text{H}_{17}\text{O}$	
Caprylic	„	$\text{CH}_3\text{O} + 7 \text{CH}_2 = \text{C}_8\text{H}_{19}\text{O}$	
Palmitic	„	$\text{CH}_3\text{O} + 15 \text{CH}_2 = \text{C}_{16}\text{H}_{33}\text{O}$	
Cerotic	„	$\text{CH}_3\text{O} + 26 \text{CH}_2 = \text{C}_{27}\text{H}_{55}\text{O}$	
Melissic	„	$\text{CH}_3\text{O} + 29 \text{CH}_2 = \text{C}_{30}\text{H}_{61}\text{O}$	

Again, we may take another compound as point of departure. *Formic acid* is a body long since discovered to be secreted by the ant. By adding an atom of carbon and two of hydrogen to this acid we arrive at *acetic acid*, which we have already met with this evening as one of the products of the oxidation of olefant gas. By the successive accumulation, within the molecule of this acid, of similar quantities of carbon and hydrogen, a long series of acids is formed, including some of the most interesting compounds with which the chemist has to deal: *butyric acid*, contained in butter; *valeric*, the active constituent of the valerian root; *caproic* and *caprylic*, obtained from goat's fat; *ænanthylic*, from castor oil; *pelargonic*, the odoriferous principle of *pelargonium roseum*; *rutic*, the product of oxidation of oil of rue; *palmitic*, contained in palm oil and in spermaceti; *margaric* and

stearic, constituents of the majority of animal fats; *cerotic* and *melissic acids*, lastly, occurring in the several waxes.

Increment of Carbon.

ACIDS.

Formic acid	. .	$C H_2 O_2$	
Acetic	„ . .	$C H_2 O_2 +$	$CH_2 = C_2 H_4 O_2$
Propionic	„ . .	$C H_2 O_2 +$	$2 CH_2 = C_3 H_6 O_2$
Butyric	„ . .	$C H_2 O_2 +$	$3 CH_2 = C_4 H_8 O_2$
Valeric	„ . .	$C H_2 O_2 +$	$4 CH_2 = C_5 H_{10} O_2$
Caproic	„ . .	$C H_2 O_2 +$	$5 CH_2 = C_6 H_{12} O_2$
Enanthylic	„ . .	$C H_2 O_2 +$	$6 CH_2 = C_7 H_{14} O_2$
Caprylic	„ . .	$C H_2 O_2 +$	$7 CH_2 = C_8 H_{16} O_2$
Pelargonic	„ . .	$C H_2 O_2 +$	$8 CH_2 = C_9 H_{18} O_2$
Rutic	„ . .	$C H_2 O_2 +$	$9 CH_2 = C_{10} H_{20} O_2$
Lauric	„ . .	$C H_2 O_2 +$	$11 CH_2 = C_{12} H_{24} O_2$
Cocinic	„ . .	$C H_2 O_2 +$	$12 CH_2 = C_{13} H_{26} O_2$
Myristic	„ . .	$C H_2 O_2 +$	$13 CH_2 = C_{14} H_{28} O_2$
Benic	„ . .	$C H_2 O_2 +$	$14 CH_2 = C_{15} H_{30} O_2$
Palmitic	„ . .	$C H_2 O_2 +$	$15 CH_2 = C_{16} H_{32} O_2$
Margaric	„ . .	$C H_2 O_2 +$	$16 CH_2 = C_{17} H_{34} O_2$
Stearic	„ . .	$C H_2 O_2 +$	$17 CH_2 = C_{18} H_{36} O_2$
Cerotic	„ . .	$C H_2 O_2 +$	$26 CH_2 = C_{27} H_{54} O_2$
Melissic	„ . .	$C H_2 O_2 +$	$29 CH_2 = C_{30} H_{60} O_2$

The action of the acids just examined upon the groups of alcohols previously studied gives rise, as is well known, to the class of bodies called compound ethers. On arranging some of the numerous bodies belonging to this group into a series in which the carbon rises, atom by atom, we find, in exact accordance with our former observations, that the accession of one atom of carbon involves the simultaneous introduction of two atoms of hydrogen :—

Increment of Carbon.

COMPOUND ETHERS.

Formate of Methyl	. .	$C_2 H_4 O_2$	
Formate of Ethyl	. .	$C_2 H_4 O_2 +$	$CH_2 = C_3 H_6 O_2$
Acetate of Ethyl	. .	$C_2 H_4 O_2 +$	$2 CH_2 = C_4 H_8 O_2$
Butyrate of Methyl	. .	$C_2 H_4 O_2 +$	$3 CH_2 = C_5 H_{10} O_2$
Butyrate of Ethyl	. .	$C_2 H_4 O_2 +$	$4 CH_2 = C_6 H_{12} O_2$
Acetate of Amyl	. .	$C_2 H_4 O_2 +$	$5 CH_2 = C_7 H_{14} O_2$

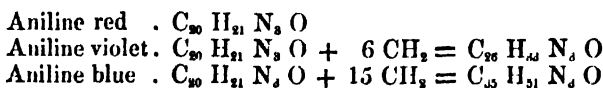
All these substances present more or less general interest. The powerful, and in some cases almost repulsive odours which the compound ethers possess may be tamed down by dilution, so as to render these substances useful, and indeed extensively applied, substitutes for natural essences. *Formate of methyl*, the simplest of all compound ethers, like the next term, *formate of ethyl*, has received some applications in the flavouring of inferior varieties of rum. *Acetate of ethyl*, familiar to everyone as acetic ether, is used for "improving" certain wines; the *butyrate of methyl* and *ethyl*, substances which undiluted possess an almost overwhelming, and by no means attractive, odour, exhale, when dissolved in an appropriate amount of spirits of wine, the finest perfume of the pine apple; *acetate of amyl*, lastly, the final term of our series, exhibits the peculiar aroma of the jargonelle pear in so high a degree that it is now extensively manufactured to flavour the well-known pear drops of our confectioners.

I must not, however, dilate too much on the odoriferous qualities of the compound ethers; here, indeed, we are concerned with these substances only in so far as they afford additional evidence in favour of our speculations respecting the growth of carbon in a series of carbon compounds.

One more illustration and we shall have done with this part of our inquiry. In a lecture delivered some time ago in this theatre, I had the honour of submitting to the members of the Royal Institution a brief account of *Mauve* and *Mugenta*, the remarkable coal-derived colouring matters which have sprung from the happy union of industry and science in our times. May I be permitted once more to call your attention for a moment to the group of tinctorial ammonias? *Aniline red*, or *rosaniline*, as it is called by chemists, is convertible by certain processes into beautiful *violet*, and even *blue* colouring matters. This conversion invariably involves addition of carbon to the molecule of rosaniline. By its conversion into certain varieties of violet, the red fixes six atoms of carbon; by its transition into certain bluish shades, not less than fifteen atoms of carbon are assimilated. In what manner does this increment of carbon affect the amount of hydrogen? Inspection of the diagram teaches us that aniline violet contains $12 = 2 \times 6$ atoms of hydrogen more than aniline red, and that the transition of red into blue is attended by an accession of as many as $30 = 2 \times 15$ atoms of hydrogen.

Increment of Carbon.

COLOURING MATTERS.



In the preceding remarks I have submitted to you an extensive series of examples taken from different pages in the vast volume of chemistry, which appear to indicate that oxygen combines atom by atom, that nitrogen enters the compound freighted with one atom of hydrogen, and lastly, that carbon cannot join the concern except with a capital, so to speak, of two atoms of hydrogen. Supposing for a moment that this rule could be established without a single exception throughout the whole range of chemistry, are we enabled to assign a probable reason for this peculiar behaviour of the oxygen, nitrogen, and carbon atoms?

To answer this question we must commence by considering for a few moments some of the simplest compounds of the elements concerned.

Here we have four glass tubes surmounted by large globes. The first of these contains *hydrochloric acid*, the compound of hydrogen with chlorine; the second did contain the vapour of *water*, the compound of hydrogen with oxygen, but which is now condensed to small drops of fluid water deposited on the sides of the globe; in the third vessel we have the compound of hydrogen with nitrogen, *ammonia*; while the last is filled with one of the compounds of carbon with hydrogen, *viz. marsh-gas*.

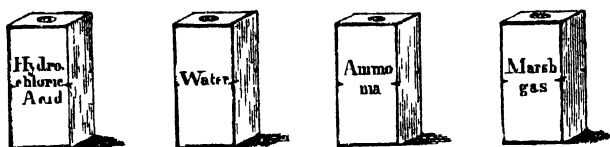
These four compounds, all transparent and colourless, we distinguish by the simplest experiments. On opening the sealed tubes of the vessels under mercury no change takes place in the case of hydrochloric acid, ammonia, and marsh-gas, while the mercury immediately rises and fills the globe containing the condensed water vapour. When the remaining three vessels are now raised, so as to allow the broken points of the tubes to dip in a layer of water floating on the mercury, the liquid rushes into the globes containing hydrochloric acid and ammonia; of the two solutions thus produced by the absorption of the gases, the one containing the hydrochloric acid reddens blue litmus; the other, formed by the absorption of the ammonia, changes red litmus to blue. The marsh-gas differs from all the others by its insolubility and by its inflammability. Indeed, if the globe be broken and a light applied, the gas burns with a feebly luminous flame.

Nor are the differences observed in the structure of these four hydrogen compounds less characteristic, although they cannot be so easily demonstrated by experiment, at all events within the limited time at my disposal. Indeed, to give you an idea of their structure which is necessary for our purposes, I must be permitted to borrow a leaf from the book of the Wizard of the North, and to avail myself of a simple mechanical contrivance devised for that purpose. Let these four tin boxes represent two volumes of hydrochloric acid, water-gas, ammonia, and marsh-gas.

We wish to know the quantities of hydrogen contained in two volumes of each of these four bodies, and we find that out of the two volumes of hydrochloric acid we are enabled to pull *one* volume of hydrogen; out of the same volume of water-vapour, *two* volumes of

hydrogen ; out of the same volume of ammonia, by a mechanical contrivance of the simplest description, *three* volumes of hydrogen ; and lastly, out of two volumes of marsh-gas, *four* volumes of hydrogen.

FIG. 1.

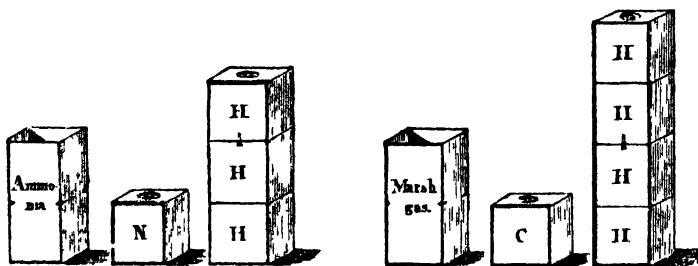


We now require to know the quantities of the other elements present in the four compounds under examination, and again we extract from the original two volumes of hydrochloric acid *one* volume of chlorine ; from the two volumes of water-vapour, *one* of oxygen , from the two of ammonia, *one* of nitrogen ; and lastly, from the two volumes of marsh-gas, an amount of carbon which *provisionally* I may be permitted to represent by *one* volume, since, owing to the non-volatility of the carbon, the volume of the carbon vapour has not yet been ascertained.

FIG. 2.



FIG. 3



The information mechanically conveyed to us by our tin boxes is embodied in the following diagram, which is, moreover, calculated to enlarge our views respecting the combining powers of the elements chlorine, oxygen, nitrogen, and carbon.

FIG. 4.

Hydrochloric Acid.



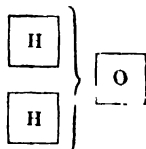
Chlorine (Molecule).



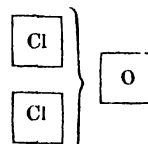
Chloride of Sodium.



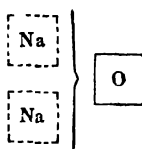
Water.



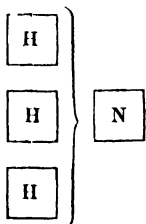
Hypochlorous Anhydride.



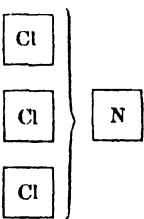
Oxide of Sodium.



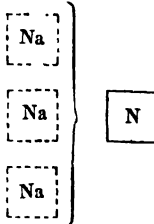
Ammonia.



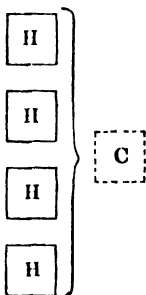
Chloride of Nitrogen.



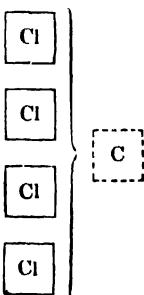
Trisodamide.



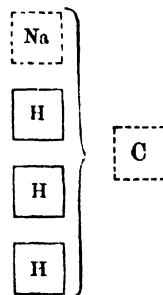
Marsh Gas.



Tetrachloride of Carbon.



Sodium-methyl.



In the second column of the diagram are given the compounds of these four elements with chlorine, and exactly as we have seen them combining respectively with 1, 2, 3, and 4 volumes of hydrogen, we now find them associated with 1, 2, 3, and 4 volumes of chlorine. Again, in the third column we have the series of sodium compounds of these elements, and although in this case we must be careful not to speak of volumes of sodium-vapour, since chemists have not as yet obtained sodium-gas in the pure state, we perceive at all events from the diagram that one volume of oxygen fixes exactly twice, one volume of nitrogen exactly three times the quantity of sodium which is combined with one volume of chlorine.

The study of elementary gases has led chemists to the unanimous opinion that equal volumes of these different gases contain an equal number of the smallest particles or atoms. Numerous theoretical considerations and numerous experimental inquiries inevitably lead to this result, which is now a generally received truth. If equal volumes of different gases be submitted to the same pressure they contract to an equal extent, and if they are equally heated they expand equally.

We have here an apparatus so disposed as to permit us to establish this point experimentally. It consists of a kind of double U tube, with one long and simple limb, and one short limb bifurcated into two branches, each of which is provided with a stopcock. These short limbs are moreover surrounded by a glass cylinder. Near the bottom of the apparatus another stopcock is placed, enabling us to empty the apparatus. The three limbs of the instrument being filled with mercury, we introduce into the stopcocked limbs the gases to be examined, into the one hydrogen and into the other oxygen, care being taken to have as nearly as possible equal volumes of the two. These volumes being marked by caoutchouc rings we pour mercury into the long open limb, and we find that the mercury column thus obtained compresses the two gases to an exactly equal extent. Again, on letting out mercury through the bottom stopcock, so as to lower the column and diminish the pressure, we observe that both hydrogen and oxygen undergo equal dilatation. And so again we are enabled to prove the expansion and contraction of the two gases to be equal, if the glass cylinder surrounding the bifurcated limb of the apparatus be filled alternately by hot steam or cold air.

Now, if equal volumes of the elementary gases contain an equal number of atoms, it is obvious that

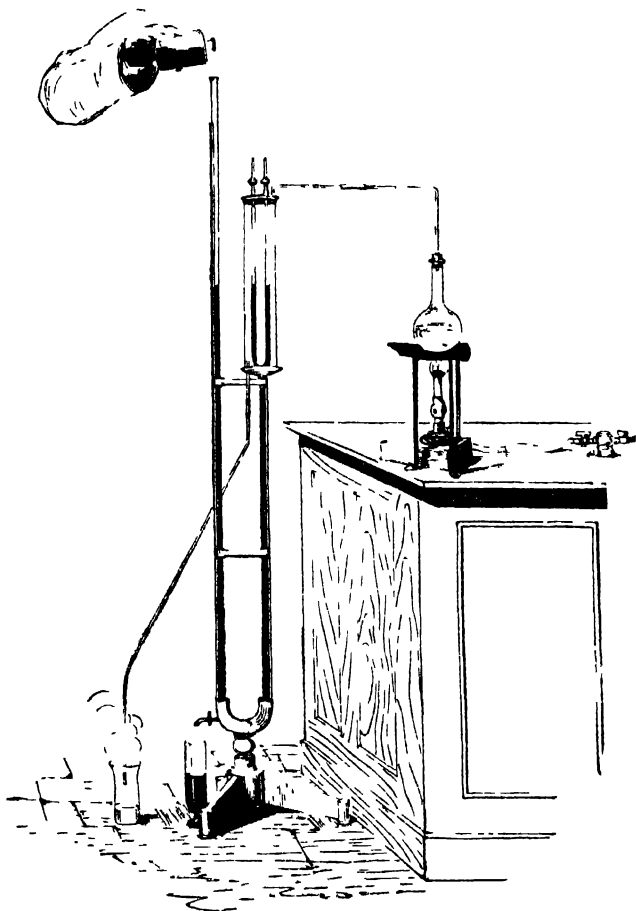
The chlorine atom combines with	1	atom of hydrogen,
„ oxygen	„	2 atoms
„ nitrogen	„	3 „

and since there are reasons for believing that the quantity of carbon existing in two volumes of marsh-gas (but which, as I have pointed out, has never been obtained in the gaseous state) represents the carbon atom, we may add that

The carbon atom combines with 4 atoms of hydrogen.

And in a similar manner we may say that the atoms of the four elements in question, when uniting with chlorine, are found to be capable of fixing 1, 2, 3, or 4 atoms of chlorine.

FIG 5.



These observations which, if time permitted, might be considerably expanded, lead us to a very important distinction of elementary atoms, which is based upon their *atom-fixing capabilities*, more shortly expressed, upon their *combining powers*.

If the atom-fixing power of the chlorine atom be taken as standard of comparison, we are compelled to assign to the oxygen atom the two-fold, to the nitrogen atom the threefold, and to the carbon atom the

fourfold atom-fixing power: or, looking at this question from a different point of view, we find that one atom of oxygen is performing the work of two atoms of chlorine; one atom of nitrogen, that of three; one atom of carbon, lastly, that of four atoms of chlorine. Hence we distinguish the chlorine atom as *univalent*, the atom of oxygen as *bivalent*, that of nitrogen as *trivalent*, and, lastly, the carbon atom as *quadrivalent*.

With the recognition of these different combining powers in elementary atoms we have made a very considerable step towards the solution of the question which presented itself in the earlier part of this discourse, *viz.*: How is it that the oxygen atom is fixed directly, the nitrogen atom together with one, the carbon atom, lastly, together with two atoms of hydrogen? This peculiar mode of combination is the necessary consequence of the special atom-fixing capabilities of the oxygen, nitrogen, and carbon atoms; and this I believe I can show you by a very simple contrivance. I am again tempted to rely entirely on mechanical means of elucidating the subject, and I will on this occasion, with your permission, select my illustration from that most delightful of games, *croquet*.

Let the croquet balls represent our atoms, and let us distinguish the atoms of different elements by different colours. The white balls are hydrogen, the green ones chlorine atoms; the atoms of fiery oxygen are red, those of nitrogen, blue; the carbon atoms, lastly, are naturally represented by black balls. But we have, in addition, to exhibit the different combining powers of these atoms. This we accomplish by screwing into the balls a number of metallic arms (tubes and pins), which correspond respectively to the combining powers of the atoms represented, and which, while constituting an additional feature of distinction, enable us at the same time to join the balls and to rear in this manner a kind of mechanical structures in imitation of the atomic edifices to be illustrated. Thus the hydrogen and chlorine atoms, which are *univalent* atoms, have each *one* arm, representing *one* combining or attraction unit; the atom of oxygen, a *bivalent* atom, has *two*, representing *two* attraction units; while the nitrogen and carbon atoms, respectively *trivalent* and *quadrivalent*, are provided with *three* and *four* arms, indicating the *three* and *four* combining units respectively distinguishing these atoms.

FIG. 6.



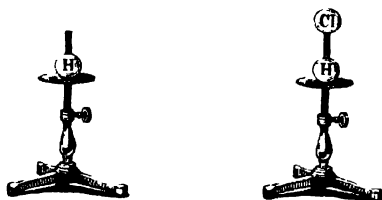
Of the newly-formed building materials let us make a preliminary trial in constructing the four hydrogen compounds just examined.

We start with four appropriately disposed stands as a foundation, on each of which we place a hydrogen sphere as the first building stone.

On one of these hydrogen atoms we fix a chlorine atom by inserting

the solid arm of the former into the tubular arm of the latter, we have thus constructed a molecule of hydrochloric acid. The attraction units of the two elements are saturated; we can add no more; the building is finished.

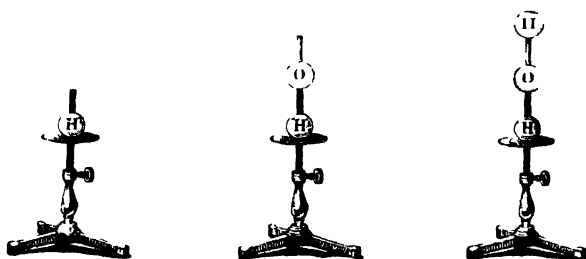
FIG. 7.



Formation of HYDROCHLORIC ACID

The next hydrogen atom we combine in a similar manner with an atom of oxygen, but no closed molecule is thus produced. One of the attraction units of the oxygen is not yet saturated, as indicated by the one arm remaining uncovered. Only by fixing upon this arm a second hydrogen atom we saturate this second attraction unit also. The closed water molecule is a finished building.

FIG. 8.

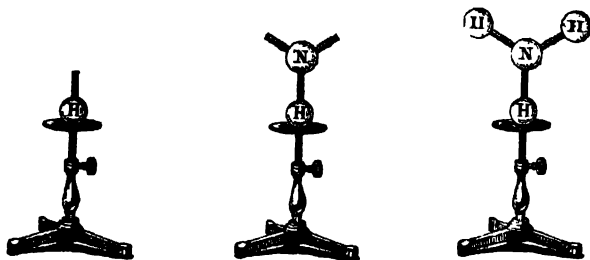


Formation of WATER.

Again, an atom of nitrogen is inserted into the hydrogen atom on our third stand; the two nitrogen arms which are left exposed sufficiently indicate that two attraction units remain unsaturated, and have accordingly to be provided with two atoms of a univalent element.

If this univalent element be hydrogen, the finished structure is the molecule of ammonia.

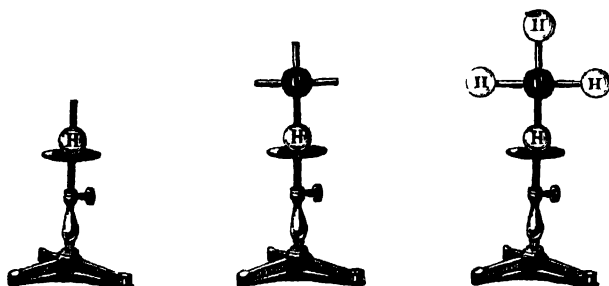
FIG. 9.



Formation of AMMONIA

In a similar manner, lastly, we perceive that when a four-armed carbon atom is fixed upon the hydrogen atom, the three combining units remain unsaturated, and that the construction of a closed molecule of marsh-gas can be accomplished only by the accession of three atoms of hydrogen.

FIG. 10.



Formation of MARSH-GAS

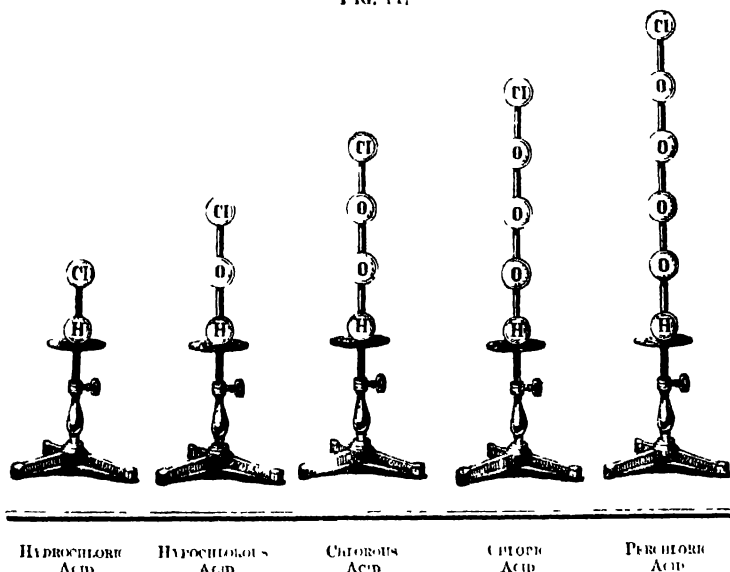
At this stage we may appropriately resume the question suggested by the earlier considerations of this evening. The facility with which our newly-acquired building material may be handled, enables us to construct even some of the more complicated substances involved in these considerations.

And first, the building up of the oxygen compounds of hydrochloric

acid may be attempted. On opening the molecule of *hydrochloric acid*, two attraction units (one belonging to the hydrogen atom, and the other to the chlorine atom,) are liberated; they are exactly equal to the two attractions of the bivalent atom of oxygen. With the insertion of an oxygen atom we perceive that the molecule is closed again, no uncovered arm projects, no attraction remains unbalanced. This new molecule—we call it *hypochlorous acid*—we open again: again two attraction units are liberated and saturated by a second atom of bivalent oxygen. The molecule of hypochlorous acid is thus converted into the molecule of *chlorous acid*. The insertion of one or two more oxygen atoms under exactly similar circumstances would give rise to the formation of the molecules respectively of *chloric* and of *perchloric* acids.

We are thus enabled, by availing ourselves exclusively of oxygen as building material, to convert the two-storied molecule of hydrochloric acid successively into a three-, four-, five-storied molecule, and ultimately even into the six-storied molecule of perchloric acid; and there is no reason why a happy experimentalist, by using additional and more complicated scaffolding, should not succeed in raising still loftier structures.

FIG. 11.

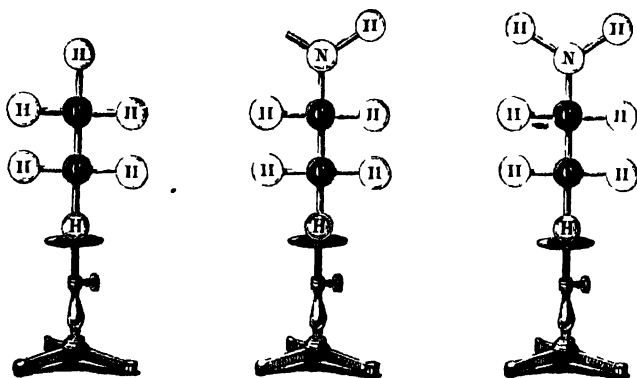


The reason why, when uniting with a compound, oxygen joins this compound, atom by atom, is now obvious. By opening a finished molecule, two attraction units are set free; these may be balanced by

one atom of oxygen, but also by a chain of two, three, four, in fact, of any number of oxygen atoms. Of the two, four, six, eight, &c. attraction units possessed by one, two, three, and four atoms of oxygen, two, four, and six units are consumed in linking these atoms into a chain, so that only two units, one at each end of the chain, remain at our disposal, and may be used in closing up again the broken molecule.

Not less satisfactory is the information supplied by the consideration of a series of nitrogenetted compounds. In *hydride of ethyl* we possess a molecule containing two atoms of carbon and six atoms of hydrogen. With the view of introducing an atom of nitrogen we break this molecule. A glance at our model shows us at once, that by inserting between the fragments a nitrogen atom only, we are unable to reproduce a finished building, for of the three attraction units with which the nitrogen atom enters, two only are saturated; one remains unsaturated, indeed one nitrogen arm projects uncovered. It is only by addition of another atom of hydrogen that the closed

FIG 12.

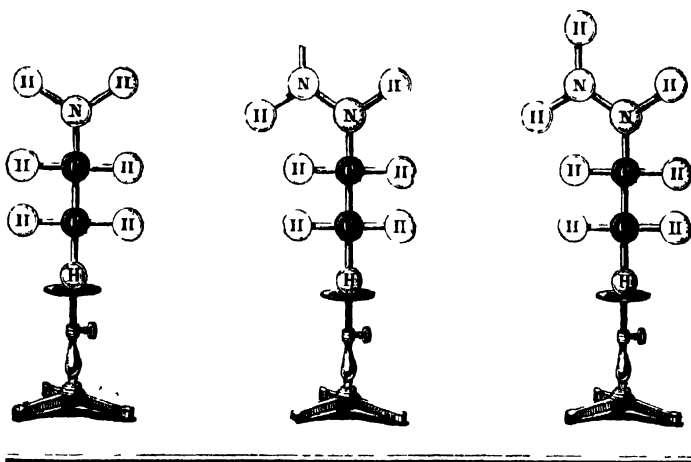


HYDRIDE OF ETHYL.

ETHYLAMINE

molecule of *ethylamine* is formed. This molecule again we open, again we introduce an atom of nitrogen, and again we observe that one attraction unit of the newly-added atom remains unsatisfied, and that an additional atom of hydrogen is required for the transformation of ethylamine into *ethylene-diamine*, and so again when ethylene-diamine is to be converted into *vinyltriamine*.

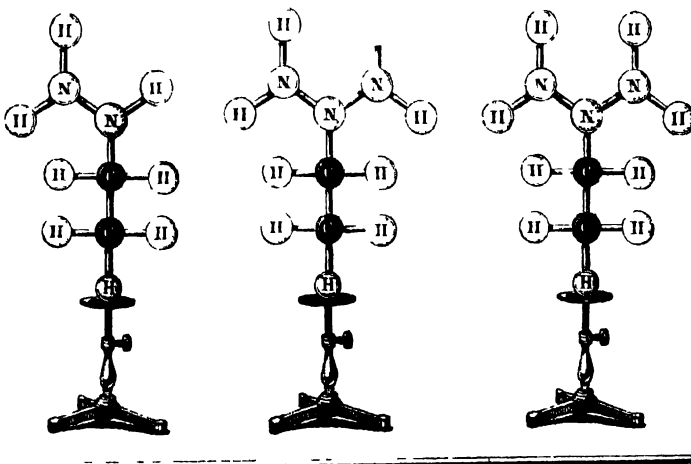
FIG. 13.



ETHYLAMINE

ETHYLENE DIAMIDE.

FIG. 14.



ETHYLENE DIAMINE

VINYLTRIAMINE.

Why the nitrogen atom does not combine directly, like the atom of oxygen, but always carries an atom of hydrogen along with it, is now likewise demonstrated. If a finished molecule be broken for the reception of a new atom, the number of combining units liberated is always two. This, as we have seen, is the number which is saturated by one atom of oxygen, or by a chain of oxygen atoms; but when these two attraction units are saturated by a nitrogen atom, one of the attraction units of the nitrogen remains free; when they are saturated by a chain of nitrogen atoms, the number of attraction units remaining free must be equal to the number of atoms composing the nitrogen chain.

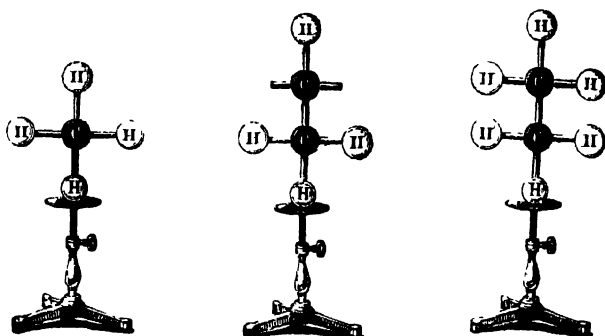
The experience acquired in the study of the oxygen and nitrogen compounds has prepared us for the examination of the increment of carbon. In fact, it is only necessary to apply the method hitherto followed to one of the series of carbon compounds already reviewed, to enable us to understand why the carbon atom is assimilated, not atom by atom, like the oxygen atom, not associated with *one* atom of hydrogen like the nitrogen atom, but associated with *two* atoms of hydrogen.

We have only to remember that the carbon atom saturates *four* combining units, while the nitrogen atom saturates only *three*. Now, we have seen that the rupture of a molecule involves the liberation of two attraction units. The saturation of these two units by the trivalent nitrogen atom left $3 - 2 = 1$ attraction unit disengaged; in a similar manner, if the saturation be effected by the quadrivalent carbon atom, $4 - 2 = 2$ attraction units must remain unsaturated.

The reason why the carbon atom, when fixed by compounds, associates with two atoms of hydrogen,—why so large a number of carbon compounds differ by CH_2 , or a multiple thereof,—in fact, the relation in the composition of numerous series of carbon compounds which is designated by the term *homology*, is now intelligible.

In attempting to illustrate this behaviour of the carbon atom by our mechanical models, we conveniently select the molecule of *marsh-gas*, the simplest compound of carbon and hydrogen, as the foundation of our edifice. This molecule we open for the insertion of a second carbon atom. The two attraction units liberated by the rupture of the molecule are saturated by two of the attraction units of the quadrivalent carbon atom, two attraction units of which remain unsaturated. Indeed two carbon arms remain uncovered, on which we forthwith fasten two atoms of hydrogen. The transformation of marsh-gas into *hydride of ethyl* is thus accomplished.

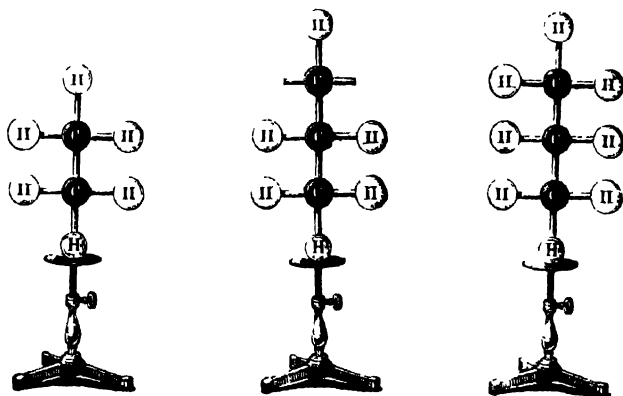
FIG. 15.

M^o, 1889, 1890.

HYDRIDE OF ETHYL.

We open again for the reception of another atom of carbon, and this third atom joins under exactly the same circumstances, carrying along with it into the new molecule, the spaces, so to speak, for two additional atoms of hydrogen. Hydride of ethyl in this manner becomes *hydride of propyl*.

FIG. 16.

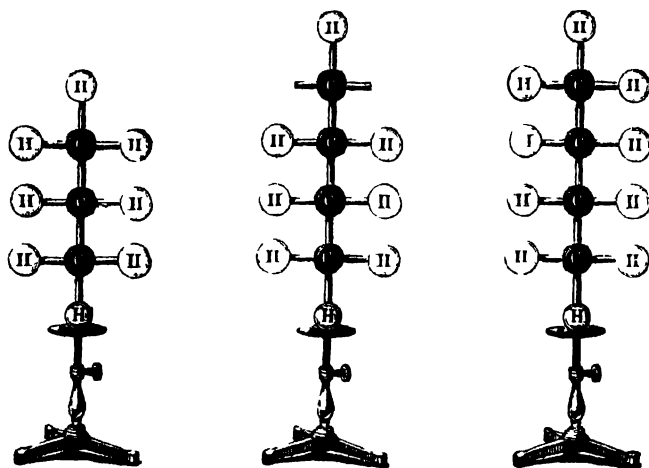


HYDRIDE OF ETHYL.

HYDRIDE OF PROPYL.

Hydride of propyl, by the accession of a fourth carbon atom, is converted into *hydride of butyl*.

FIG. 17.



HYDRIDE OF PROPYL

HYDRIDE OF BUTYL

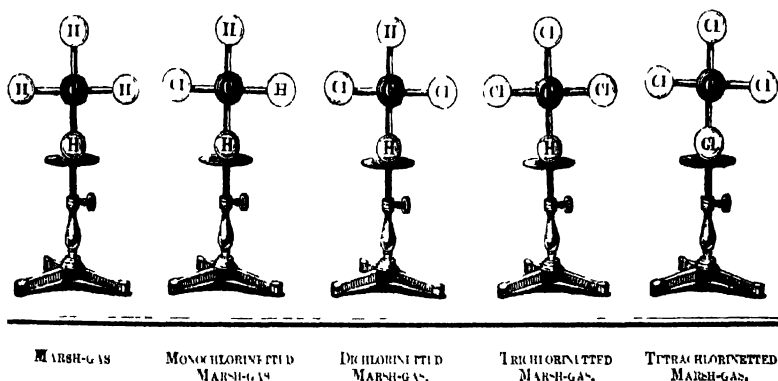
It is scarcely necessary to expand these illustrations, and if I venture to raise up a few more of these mechanico-chemical edifices, it is because I want to show you that our building stones are available for many other purposes.

Hitherto we have been satisfied with examining under what conditions the atoms of oxygen, of nitrogen, and carbon are received into molecular structures. The question now presents itself, on what terms the chlorine atom is allowed to join?

The model of the marsh-gas molecule is still before us. Let us open this molecule for the reception of a chlorine atom. Two attraction units are thus liberated; but the chlorine atom is univalent. Accordingly two atoms of chlorine are required, one of which combines with the hydrogen atom which we remove from the marsh-gas, converting it into hydrochloric acid which separates, while the other chlorine atom joins the rest of the molecule of marsh-gas. The new molecule, *monochlorinettid marsh-gas*, may be looked upon as marsh-gas in which one atom of chlorine holds the position originally occupied by the hydrogen atom. We are thus led up to the recognition of new conditions of combination, conditions which have not as yet attracted our attention this evening, but which unfold to us one of the

most important principles of modern chemistry, the principle of *substitution*. The monochlorinetted marsh-gas, which is a liquifiable gas, when again submitted to the action of chlorine loses a second, a third, and lastly, a fourth atom of hydrogen in the form of hydrochloric acid, giving rise to the formation of *dichlorinetted*, *trichlorinetted* marsh-gas, better known as *chloroform*, and, lastly, of *tetrachlorinetted* marsh-gas or *tetrachloride of carbon*, *i. e.* marsh-gas in which the four atoms of hydrogen are displaced by an equal number of chlorine atoms.

FIG. 18.

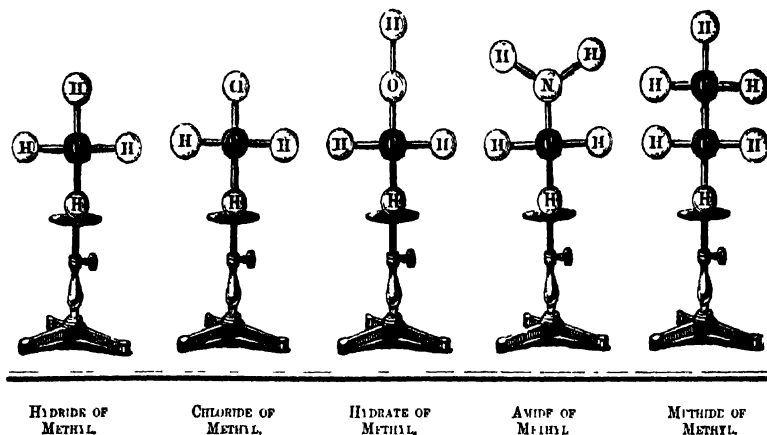


Our information regarding the combining powers of atoms is thus materially expanded. Having already learnt that when entering into a molecular structure the carbon atom associates with *two* atoms of hydrogen, the atom of nitrogen with *one* atom of hydrogen, that the oxygen atom combines *directly*, we now find that the chlorine atom combines only by *substitution*, *i. e.* when a space has become vacant in the molecule by the expulsion of hydrogen.

The rest of the marsh-gas molecule remaining after the introduction of one atom of chlorine, and consisting of one atom of carbon, combined with three atoms of hydrogen, is frequently designated by the name of *methyl*. The aggregate of atoms CH_3 , the radical methyl, may be traced in all the compounds obtainable from marsh-gas by the insertion of other atoms. Thus by the assimilation of an oxygen atom, marsh-gas becomes methylic alcohol, *i. e.* water in which one atom of hydrogen is displaced by methyl, by the absorption of nitrogen with its tributary hydrogen, it becomes methylamine, *i. e.* ammonia in which one atom of hydrogen is displaced by methyl; by the incorporation of an atom of carbon, lastly, with its pair of hydrogen atoms, the marsh-gas molecule is converted into methyl-marsh-gas, *i. e.* marsh-gas in which an atom of hydrogen is displaced by methyl.

At one time the exertions of chemists were anxiously directed to the isolation of the atomic aggregate CH_3 , of the radical methyl,

FIG. 19.



from one or other of the methylic bodies just enumerated. The facility with which the chlorine atom in chloride of methyl may be exchanged for other atoms, leaving the aggregate of carbon and hydrogen, which we call methyl, perfectly intact, the mobility of one of the hydrogen atoms in methylic alcohol, and of two of the hydrogen atoms in methylic ammonia, the possibility of replacing even the oxygen and the nitrogen in these compounds, without affecting the methyl, the stability, lastly, of methylic marsh-gas, containing as it does the whole of its carbon and the whole of its hydrogen in the form of methyl, all these circumstances appeared to indicate the probability of the separate existence of methyl. Why all attempts to separate the atom group CH_3 have remained unsuccessful; why methyl could not be caught; why, ultimately when Dr. Frankland's masterly experiments appeared to have precluded all chance of escape, despairing methyl combined with itself, surrendering as methylic marsh-gas or methyl-methyl; why, in fact, it would appear to be an essential character of methyl not to have a separate existence: all these questions are readily answered by our *croquet balls*, which exhibit us methyl as an unfinished molecule, capable of conversion into the finished molecules, hydride, chloride, hydrate, amide and methide of methyl, but not capable of existing as a molecular fragment with imperfectly balanced attractions.

Taking a farewell glance at the results we have elicited this evening, we may fairly ask whether the experience collected on a

comparatively limited area is fully and unequivocally corroborated by the examination of a more expanded range of observations? To this question we cannot unhesitatingly give an affirmative answer. It would not be difficult to quote a number of substances, the construction of which appears to have been governed by rules of combination different from those which we have endeavoured to lay down. Indeed, we need not even go beyond the circumscribed field on which we have hitherto moved to meet with prominent cases of exception.

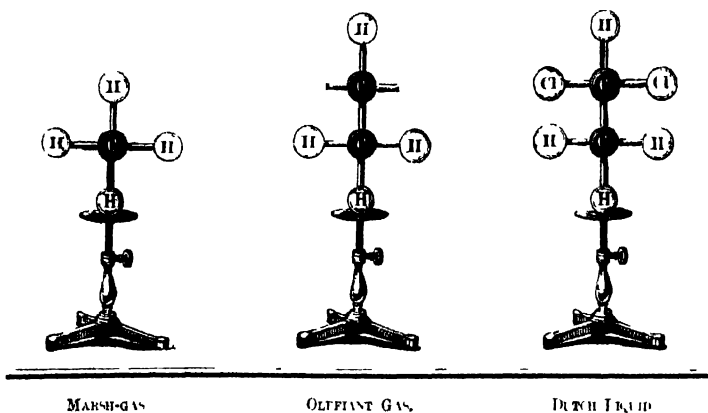
Among the several compounds of carbon and hydrogen which have passed through our hands this evening, we remember the two simplest, *marsh-gas* and *olefiant gas*. In the marsh-gas molecule CH_4 , we have the carbon atom completely saturated with hydrogen; by the entrance of a second carbon atom, with its two accessory hydrogen atoms, we saw the molecule of marsh-gas, or hydride of methyl, converted into the molecule of hydride of ethyl C_2H_6 .

The formula of olefiant gas, C_2H_4 , places its molecule midway between the molecules of marsh-gas and hydride of ethyl. Comparing olefiant gas with marsh-gas, we find that it contains one of carbon more than the latter, the number of hydrogen atoms being equal in both substances. Contrary, then, to the rule on which we hitherto relied, we find that the carbon atom transforming the marsh-gas molecule into the molecule of olefiant gas, enters *without* carrying along the two atoms of hydrogen, which we had accustomed ourselves to consider as the inseparable companions of the carbon atom on such occasions. While frankly admitting that in olefiant gas we meet with the first exception to a rule hitherto unbroken, we are entitled to inquire whether there are no means of explaining this anomalous construction of the olefiant gas molecule. Let us again apply to the models which, in a measure, have assisted us in constructing the rule; perhaps they may help us also in elucidating the exception.

In building up the molecule of olefiant gas by the insertion into the marsh-gas molecule of one atom of carbon only, we obtain what hitherto we would have called an *unfinished* molecule, *i.e.* a molecule in which two of the attraction units of the second carbon atom are unsatisfied. Indeed, a glance at our model shows us that two carbon arms project uncovered. We are thus led to inquire whether unfinished molecules, *i.e.* molecules in which a certain number of attraction units remain unbalanced, are capable of a separate existence. This question is accessible to experiment. Olefiant gas, indeed, possesses all the characters which, granting for argument's sake the possibility of its existence, we are inclined to attribute to an unfinished molecule. In the cases hitherto considered, we saw the chlorine atom, when admitted into a molecular structure, always entering *with substitution*, hydrogen separating from the chlorinetted molecule in the form of hydrochloric acid. In this manner we succeeded in transforming the marsh-gas molecule successively into monochlorinetted, di-, tri-, and, lastly, tetrachlorinetted marsh-gas or tetrachloride of carbon. Submitting, on

the contrary, olefiant gas to the action of chlorine, we find that the chlorine is fixed *directly, without substitution*, the chlorine atoms meet,

FIG. 20.



so to speak, with vacant spaces existing in the olefiant gas molecule; in order to get in, they need not expel a corresponding number of hydrogen atoms to make room for them. The compound generated is the so-called *Dutch liquid*, an oily substance first produced by an association of Dutch chemists at the close of the last century. It was the production of this oily liquid that gave rise to the name of *olefiant gas*. The number of chlorine atoms thus received directly without substitution is *two*, corresponding exactly with the number of attraction units that remained unsaturated. Any further number of chlorine atoms are found to enter by substitution, and by substitution only. Similar phenomena are observed when olefiant gas is brought into the presence of bromine. We have here a large glass vessel containing some bromine and water; the vessel, by means of a flexible tube, is connected with a gasholder filled with olefiant gas. On agitation, we see the olefiant gas rushing into the vessel as into a *vacuum*. The olefiant gas fixes two atoms of bromine, being converted into a transparent colourless liquid, the substance called *dibromide of olefiant gas*. Here again the combination takes place without substitution.

The behaviour of olefiant gas, under the influence of chlorine and bromine, elucidates the nature of its molecule. The facility with which this gas is capable of fixing two atoms of chlorine to become Dutch liquid, two atoms of bromine to become bromide of olefiant gas, and by roundabout processes two atoms of hydrogen to become hydride of ethyl—all three finished molecules—characterizes olefiant gas as a molecule interrupted in its growth, and in which the power of resuming this growth, and the limit of its final development, may

be traced by the simplest experiments. The apparently anomalous construction of the olefiant gas molecule is thus most satisfactorily accounted for. Indeed, far from disturbing the harmony of the rules of combination elicited by our inquiries, a closer examination into the nature of this compound, whilst explaining whatever appeared exceptional in its construction, leads us, on the contrary, to a loftier interpretation of these rules, to the conception of compounds, the very structure of which foreshadows the more prominent features of their chemical character.

I have selected olefiant gas as an example of a class. We remember that this substance is the first term of a long list of homologous bodies, in all of which we find similar structure combined with similar chemical properties. All these substances, and, let me add, a great variety of others, we have to regard as molecules arrested under special circumstances at a certain stage of their development, but capable, under favourable conditions, of growing again, until by the perfect balance of the atomic attractions within, they have ultimately arrived at maturity.

We have thus been led, step by step, to a distinction of a novel kind, that of *finished* and *unfinished* molecules; or, to use the more frequently employed expression, that of *saturated* and *non-saturated* compounds. I need not tell you that this distinction carries us to the threshold of a new field of research, hitherto crossed only by a small band of fearless pioneers, who are encountering difficulties on all sides. Admitting, as we are compelled to do, the existence of what we have called unfinished molecules, we inquire under what special conditions, at what special stages the growth of a molecule may be arrested? How is it that as yet the marsh-gas molecule is known only in the finished state, CH_4 , that none of the fragmentary marsh-gases, CH_3 , CH_2 , and CH , which might exist, have ever been obtained? Again, how is it that the molecule of hydride of ethyl exists, so to speak, finished and unfinished; and, lastly, that of the several fragmentary states in which this molecule might be met with, two only, namely the two states, C_2H_4 (olefiant gas), and C_2H_2 (acetylene), have ever been observed? We are thus brought face to face with some of the most deeply interesting problems of chemical mechanics, in the solution of which the exertions of chemists are engaged at the present moment. I must not, however, dwell upon the interest attached to this new line of inquiry, upon the numerous experiments which the idea of saturated and non-saturated compounds has already suggested, and on the influence it is likely to exercise on the direction of chemical investigation for some time to come.

Nor am I permitted to follow these speculations into another direction. I have to forego, more especially, the pleasure of submitting to you some of the ingenious explanations which Professor Kekulé, to whom we are greatly indebted for the development of this branch of chemistry, has advanced for the elucidation even of *saturated* com-

pounds of anomalous constitution. Tempting though the further elaboration of this subject may appear, it would lead me inevitably beyond the legitimate limits of a Friday evening lecture at the Royal Institution.

Indeed my time, and, I fear, your patience, are exhausted, and I must add but few concluding words. Your attention so kindly bestowed on my remarks will not, I trust, have been entirely thrown away, if I have succeeded in convincing you that modern chemistry is not, as it has so long appeared, an ever-growing accumulation of isolated facts, as impossible for a single intellect to co-ordinate as for a single memory to grasp.

The intricate formulæ that hang upon these walls, and the boundless variety of phenomena they illustrate, are beginning to be for us as a labyrinth once impassable, but to which we have at length discovered the clue. A sense of mastery and power succeeds in our minds to the sort of weary despair with which we at first contemplated their formidable array. For now, by the aid of a few general principles, we find ourselves able to unravel the complexities of these formulæ, to marshal the compounds which they represent in orderly series; nay, even to multiply their numbers at our will, and in a great measure to forecast their nature ere we have called them into existence. It is the great movement of modern chemistry that we have thus, for an hour, seen passing before us. It is a movement as of light spreading itself over a waste of obscurity, as of law diffusing order throughout a wilderness of confusion, and there is surely in its contemplation something of the pleasure which attends the spectacle of a beautiful day-break, something of the grandeur belonging to the conception of a world created out of chaos.

[A. W. H.]

Friday, May 12, 1865.

SIR HENRY HOLLAND, Bart. M.D. D.C.L. F.R.S. President,
in the Chair.

FREDERICK FIELD, F.R.S. L. & E.

On Magenta and its Derivative Colours.

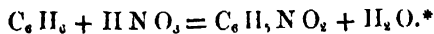
THREE years ago, in this theatre, Dr. Hofmann delivered his celebrated discourse on Mauve and Magenta, and it might seem temerity in me to trespass upon the premises of so great and distinguished a master, were it not remembered that during that interval rapid strides had been made in organic chemistry, and especially, perhaps, in the direction of the aniline colours.

Although I will endeavour to confine myself as much as possible to the immediate subject of the discourse, it will be necessary to glance for a few moments at the history of Aniline, the progenitor of nearly all the beautiful compounds you see around the table.

Aniline was discovered in the year 1826, by Unverdorben, who obtained it from the destructive distillation of indigo. A short time afterwards, Runge and Fritsche observed that by the action of strong hydrate of potash upon the dye, aniline was eliminated in far greater quantity. Indigo in small fragments is heated in a retort with a strong solution of caustic potash, and in the distillate, which consists of many products, there is found a thin and nearly colourless fluid, having a specific gravity of 1.028, a peculiar but not disagreeable odour, and a pungent biting taste. When kept for some time, even in the dark and in stoppered bottles, it assumes a darker tint, and becomes ultimately a very dark brown. Unverdorben called it "crystalline," Runge "kyonal," and Fritsche "aniline."

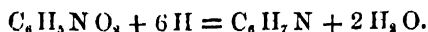
This substance is a nitrogenized base, and is capable, when combined with acids, of forming those beautiful crystalized salts, nearly all of which have been carefully examined by Dr. Hofmann and other chemists.

There are many other sources besides indigo, from which aniline may be obtained. For commercial purposes it is always prepared from nitro-benzol, a substance derived from the action of nitric acid upon benzol.



* $\text{C} = 12 - \text{O} = 16.$

Nitro-benzol when agitated with water, acetic acid, and iron, yields aniline.



Benzol was originally discovered by Mr. Faraday in 1825, in his investigations upon the gaseous products from oils subsequently obtained by the decomposition of benzoic acid by means of caustic lime. Mr. Mansfield, however, succeeded in producing it in much larger quantities from coal-tar naphtha. When the lighter portions of this compound are distilled fractionally until a constant boiling point of 180°F . is arrived at, the product consists of pure benzol, identical with the carbo-hydrogen obtained by Mr. Faraday.

From the earliest discovery of aniline it was noticed that certain oxidizing agents when mixed with a solution of its salts produced a fine violet tint. Even in minute quantities, a few drops of hypochlorite of lime render it purple. There is another test for aniline, which I will show you, and which, as far as I am aware, has not been observed previously. If the red gases obtained by the decomposition of nitric acid by starch or sugar, be passed into an aqueous solution of aniline, the liquid speedily assumes a yellow colour, owing to the formation of a new base—azophenylamine, which is gradually precipitated as a bright yellow powder. It was not, however, until the year 1856 that aniline was applied to any great practical purpose, although from the beauty of its compounds, and from its comparative accessibility, it had from the time of its discovery become a great favourite with chemists.

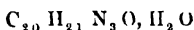
Mr. Perkin was the first who produced colour on an extensive scale from this base. He added a solution of bichromate of potash to a salt of aniline, and from the precipitate thereby produced, he isolated a magnificent purple dye, he termed "mauve," which at once became popular, and indeed at the time almost universal. It may truly be said that this discovery has identified Mr. Perkin with the aniline colours, and that he will be always associated with one of the most striking and brilliant passages in the history of chemistry as applied to the industrial arts.

It cannot be supposed that such a discovery would be allowed to rest. A mine had been opened which chemists began to explore, and in such numbers, and with such avidity and zeal, as almost to lead us to anticipate that its riches will soon be exhausted. The action of numerous bodies upon aniline and its homologues were found to be productive of colour. Nitrate of silver, nitrate of mercury, chloride of mercury, chloride of tin, arsenic acid, iodine, and many others, when heated with the base, gave a rich crimson colour, in more or less abundance; and, although it would be impossible for me to enter into a disquisition on the comparative merits of these various methods for the production of colour, I trust to be able to produce magenta, although in somewhat crude form, at this lecture table, and also to dye this tassel of silk from a solution of its salt. The reagent I will employ is iodine. A few crystals of this element are placed in a tube with about

twice their weight of aniline. Heat is at once evolved, and with the assistance of a higher temperature from the spirit lamp, you will observe, that in a few moments intense colour is developed. If a few drops are now poured into spirit, and this solution added to water, a fine rose-coloured tint will appear.

It may seem strange to those who have read Dr. Hofmann's beautiful researches upon the aniline substitutive products, his chloraniline, bromaniline, iodauniline, and a multitude of others, that he had not observed this curious reaction; and this leads me to tell you, *en passant*, for time will not allow me to dwell upon this interesting topic to-night, that aniline, when perfectly pure, does not yield any amount of colour with most of the reagents mentioned above—a most important fact discovered by Dr. Hofmann and Mr. Nicholson, and which has given rise to one of the most difficult questions which yet remain to be answered. I will simply say that it appears that there must be a homologue of aniline present with that base to produce the colour you see before you, although that homologue, *per se*, will give no colour whatever. Thus, for example, toluidine, C_7H_9N , when treated with oxidizing agents, does not produce colour; let it be mixed with aniline, and the dye is immediately developed.

The tintorial power of the salts of magenta is something marvellous. No dye that I have examined, whether from the animal, mineral, or vegetable world, can bear comparison for one moment with this crimson colour obtained from aniline. One grain in a million times its weight of water gives a pure red, in ten millions a rose pink, in twenty millions a decided blush, and even in fifty millions, with a white screen behind the vessel in which it is dissolved, an evident glow. Magenta has been carefully studied and analyzed by Dr. Hofmann, who gives us the following formula—



Although the salts of magenta are possessed of such wonderful colouring capacity, the base itself is colourless; and it is remarkable that the union of base and acid for the formation of a salt does not appear to take place in dilute solutions in the cold, at any rate not immediately. In these two vessels, one containing hot and the other cold water, an equal quantity of magenta base is added, and also an equal amount of dilute sulphuric acid. In the hot liquid colour is instantaneously developed; in the cold solution the liquid remains colourless. If now hot water be introduced to raise the temperature, you will observe at once the characteristic rose tint. It may be imagined, therefore, that having free acid in a solution of base without production of colour, it is possible to have free alkali in a coloured solution of a salt of the base without depriving it of its tint. Such is the case. If to a hot solution of acetate of magenta, for example, caustic soda is added, the colour is immediately discharged, but in a cold solution the colour remains for a long time unchanged.

Dr. Hofmann discovered, about a year ago, that when magenta, or as it is termed in chemical language, rosaniline, is heated with iodide of ethyl, a change is effected, and a substitution product formed, which was termed ethyl-rosaniline. The salts of this new base, unlike magenta, dissolve with a beautiful violet colour, and are capable of affording most remarkable manifestations. The dark violet liquid, on the addition of sulphuric acid, becomes colourless; on adding ammonia the original purple is restored. If hydrochloric acid is added in small quantities, the liquid changes to blue; if in larger quantities, to a brilliant green. When this green solution is thrown into water, so as to dilute the acid, the original violet returns.

When aniline is heated with salts of magenta, purple and blue colours are produced, all of which are now extensively employed in commerce, and afford tints of great brilliancy and beauty. The blue is perfectly insoluble in water, but readily soluble in alcohol, and is capable of dyeing both silk and wool with the greatest facility.

Mr. Nicholson patented a method a few years ago for obtaining a beautiful blue dye, soluble in water, which consisted in heating the phenyl blue in strong sulphuric acid until a drop of the semi-liquid thrown into water was found to be entirely dissolved. This compound, however, although very applicable for silks, refuses to impart its colour to wool, which may be exemplified by immersing two white tassels in the liquid—the silk is immediately dyed, while the wool remains unchanged. The effect is still more striking upon cotton. We have here the letters R. I. (the initials of the Royal Institution) worked in silk upon a cotton ground: after dipping it for a few moments in this bath the letters will become blue, and the cotton continue white.

Aniline green, which has lately become so popular, is produced by the action of aldehyde and some other deoxidizing agents upon rosaniline. This is one of the most charming colours yet discovered, but has not been (as far as its chemical nature is concerned) satisfactorily investigated. To judge of its purity of tint, it is only necessary to compare the commercial greens, prepared by various mixtures of yellow and blue, with the dye in question, to observe the infinite superiority of the latter.

Aniline brown may be formed by the action of chloride of aniline upon either magenta or violet, at a high temperature. Great destruction of colour doubtless takes place, but the brown produced is remarkably beautiful. The compound, however, is not definite, nor can it be classed among the true chemical products derived either from aniline or rosaniline.

It has been observed that magenta base is colourless: this may be said, probably, of the bases of most of the colours before you. On this white board I have traced the letters composing the word "Aniline" in seven colourless bases derived from that compound. A, in ethyl-rosaniline; N, in phenyl-violet, approaching indigo in colour; I, in phenyl-blue; L, in aniline green; I, in azo-phenylamine; N, in chrysaniline; and E, in rosaniline. On converting these bases into

salts, which is easily effected by sprinkling them with acetic acid and spirit, the seven letters should be visible in the seven colours of the rainbow—violet, indigo, blue, green, yellow, orange, and red.

I will now throw a beam from the electric lamp upon the specimens of silk on the screen, and it will be observed how much their brilliancy is increased under the influence of that pure and beautiful light. I am indebted to my kind friends, Messrs. Simpson, Maule, and Nicholson, for the various splendid specimens of dye and other aniline products, and to Messrs. Hands, Son, and Co., Coventry, the eminent silk dyers, for the array of silks so kindly furnished me for the illustration of my discourse.

[F. F.]

Friday, June 9, 1865.

THE LORD WENSLEYDALE, Vice-President, in the Chair.

EDWARD FRANKLAND, Esq. F.R.S.

PROFESSOR OF CHEMISTRY, R.I.

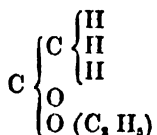
On Researches in Organic Chemistry in the Royal Institution.

THE vast crowd of organic compounds with which modern research has made us acquainted, have been grouped into a comparatively small number of families, each containing substances closely allied in chemical character. Thus, amongst others, the following families or series of organic bodies are well known and sharply defined :—

1. The Marsh-gas family.
2. The Alcohols.
3. The Organic Ammonias.
4. The Fatty, or Acetic Series of Acids.
5. The Organic Oxalic Acids, or Lactic series.
6. The Acrylic series of Acids.

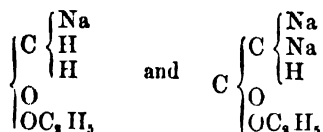
The speaker referred especially to the last three families as having been the subjects of the researches carried on in the laboratory of the Royal Institution during the past year by his friend Mr. Duppa and himself. Two of these families had already received the careful study of many chemists; the acetic having been especially illustrated by the classic researches of Kolbe and Gerhardt, whilst the lactic family had quite recently had the advantage of the equally remarkable investigations of Wurtz and Kolbe. Nevertheless, there seemed to be still some points of great interest regarding the arrangement of the atoms of these acids,—their atomic architecture, so to speak,—which had not yet received elucidation; whilst the acrylic family had hitherto enjoyed comparatively but little attention from chemists.

It had been proved by Kolbe and the speaker, nearly twenty years ago, that methyl (CH_3) is a constituent of acetic acid, and more recently that acetic acid and acetic ether are constructed upon the carbonic acid or tetratomic carbon type, the formula of acetic ether being—

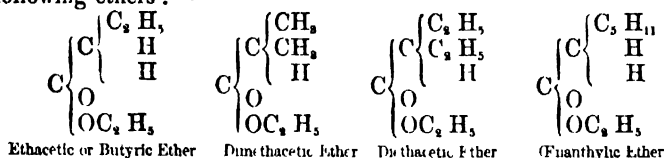


From this formula it was seen, that the radical methyl in acetic ether contained three single atoms of hydrogen combined with a tetratomic atom of carbon; and the speaker and his friend proposed to

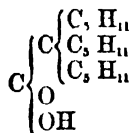
themselves the question : Can this hydrogen be replaced atom for atom by the alcohol radicals methyl, ethyl, &c.? In endeavouring to solve this problem, they availed themselves of that class of chemical reactions in which an electro-positive atom is expelled from a compound by a more electro-positive atom. Acetic ether was submitted to the action of sodium, by which two compounds of the following composition were obtained :—



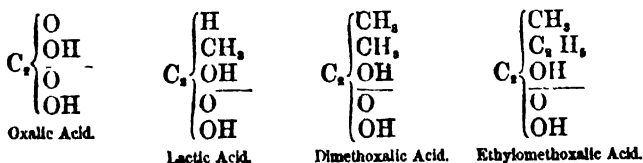
These compounds, when brought into contact with the iodides of methyl, ethyl, &c., yielded ethers, exhibiting the substitution which it was sought to obtain. In this way there had been produced the following ethers :—



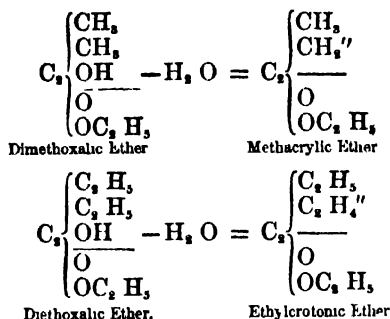
These ethers readily yielded their respective acids by contact with alcoholic solution of potash, and thus the homologous series of fatty acids could be ascended step by step, starting from acetic acid, and terminating with an acid of the composition of margaric acid, containing three atoms of amyl in the place of the three atoms of hydrogen in the methyl of acetic ether.



A similar inquiry had been instituted with regard to the family of acids of which lactic acid is a member, and it had been proved that this series of acids are derived from oxalic acid by the substitution of one atom of diatomic oxygen, in the latter by two of hydrogen, methyl, ethyl, &c. The following, amongst numerous other examples, were referred to in illustration of the relations of the primary to the derived acids :—



From the lactic family of acids access had also been gained to the acrylic family, for it had been found that the abstraction of water from the ether of an acid of the lactic family converted it into the ether of an acid of the acrylic family, thus:—



The production of these acids was not merely interesting on account of the addition of new members to the acrylic family, but their derivation from the synthesized acids of the lactic family afforded the most convincing proof of the style of architecture in which they were built up.

The investigation of the three families of acids had conclusively established between them the very simple relations exhibited in the three following formulæ:—



The speaker concluded as follows:—The day has almost gone by when the experimenter was asked the use of such investigations as these; nevertheless, it may fairly be demanded, Whither do such researches tend? What is their object? The sole object that we have had in view in these investigations has been the discovery of the laws according to which organic compounds are moulded,—those compounds, the transformation of which from one state of combination to another constitutes an essential part of the phenomena we call *life*. There is no royal road to this kind of knowledge. It is only by thus patiently and laboriously examining every part of the subject, that the combined efforts of the physicist, the physiologist, and the chemist will one day be able to solve the organic problems which at the present moment appear so unapproachable. The work before us requires a vast amount of experiment and thought. Would that more labourers

were engaged in it! How many men of leisure and ability are almost compelled to pass through life with no higher object than their own amusement. Why cannot our schools and universities furnish the next generation of such men, with the scientific knowledge necessary to enable them to take part in the glorious work of investigating Nature?

[E. F.]